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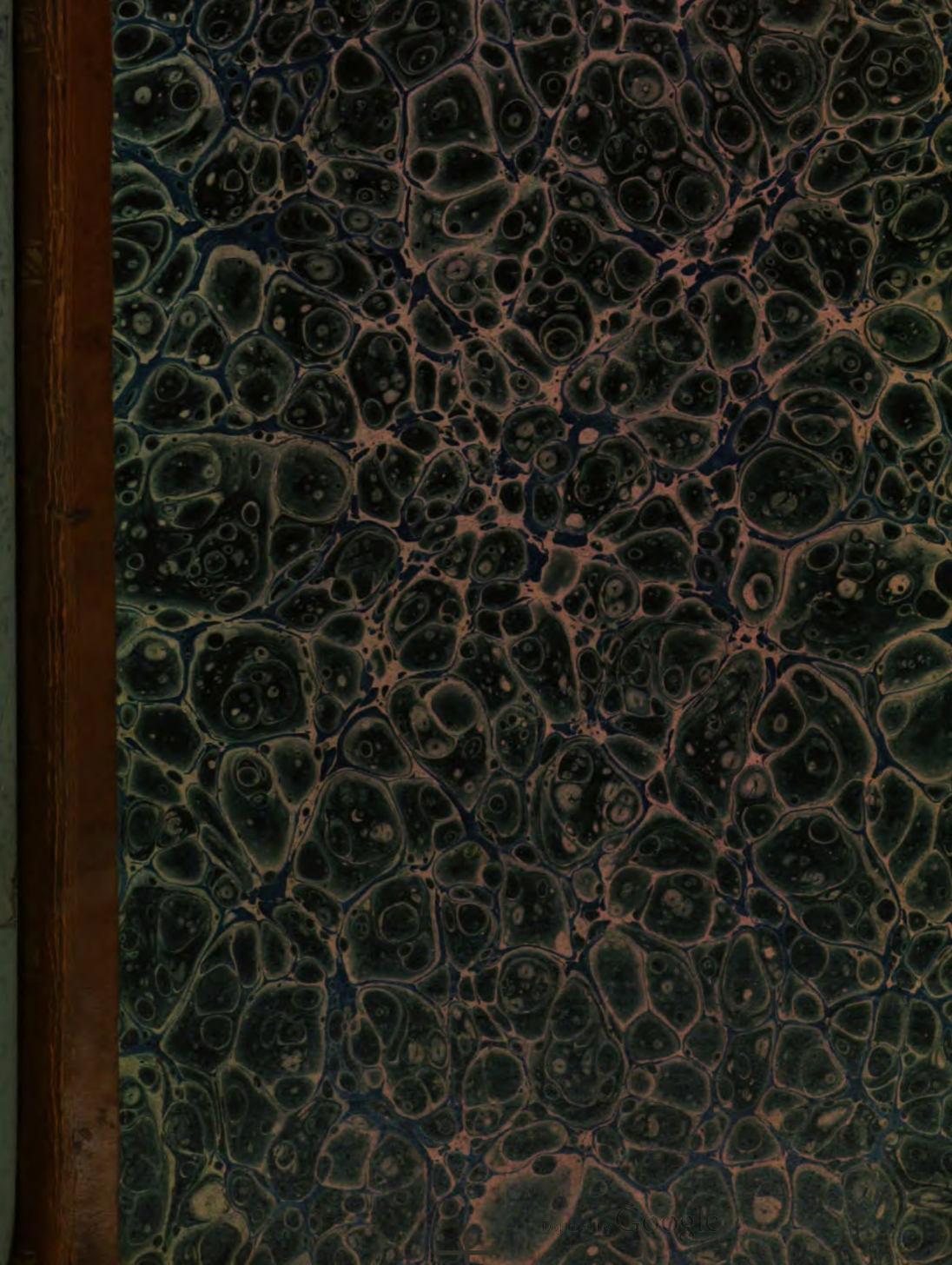
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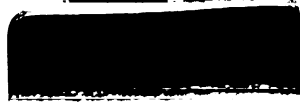
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**THE ANNALS**  
**OF**  
**ELECTRICITY, MAGNETISM,**  
**AND**  
**CHEMISTRY;**  
**AND**  
**GUARDIAN OF EXPERIMENTAL SCIENCE.**

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CONDUCTED BY

**WILLIAM STURGEON,**

**SUPERINTENDENT OF THE ROYAL VICTORIA GALLERY OF  
PRACTICAL SCIENCE, MANCHESTER;**

**FORMERLY LECTURER ON EXPERIMENTAL PHILOSOPHY AT THE HONOURABLE EAST  
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THE ANNALS  
OF  
ELECTRICITY, MAGNETISM,  
AND CHEMISTRY;  
AND  
Guardian of Experimental Science.

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JULY, 1841.

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II. *Fourth Letter on Voltaic Combinations, with reference to the Mutual Relations of the Generating and Conducting Surfaces. Addressed to MICHAEL FARADAY, Esq., D.C.L., F.R.S., Fullerian Prof. Chem. Royal Institution, &c. &c. &c. By J. FREDERIC DANIELL, F.R.S., Prof. Chem. in King's College, London.*

Received January 18,—Read January 25, 1838.

MY DEAR FARADAY,

In my second\* letter to you upon Voltaic Combinations, I suggested that, in a theoretical point of view, the most simple and perfect combination would probably consist of a solid sphere, (or rather active point) of a generating metal, surrounded by a hollow sphere of an inactive conducting metal, with an intervening liquid electrolyte; the circuit being completed by a conducting wire properly disposed for connecting the two metals. Further reflection led me to believe that a series of experiments commencing as nearly as possible with these most simple conditions of the problem, might throw some light upon the relative dimensions, positions and actions of the generating and conducting plates of voltaic combinations in general; which appeared to me not to have received all the elucidation of which they might be susceptible. The subject, it is true, has not escaped the attention of experimenters; but most of the results with which I am acquainted are so involved in the errors arising from the variable condition of the source of the current itself, which I have already pointed out, as to leave it in a very unsatisfactory state. I moreover proposed to myself, in the investigation which

\* Annals of Electricity, &c. Vol. I. p. 102.

### *Mr. Daniell on Voltaic Combinations.*

I consequently undertook, to trace the self-distribution of the force from its origin, by the indications of reduced copper, in the manner described in my first letter.\* I have thus been led to some results which I trust you will consider of sufficient importance to justify my troubling you with another communication upon the subject.

My first apparatus, which it is necessary to describe, consists of two hollow hemispheres of brass, fitting together water-tight by means of exterior flanges half an inch wide, and a collar of leather, and thus forming a sphere, the interior diameter of which is  $9\frac{1}{4}$  inches; consequently exposing a surface of about 268·8 square inches. The lower hemisphere is fitted into a frame carrying buttons, by which the upper can be securely wedged down upon it; and underneath there is a small cock, by which any liquid in the interior may be drawn off. The upper part of the upper hemisphere terminates in a tube of about one inch in length, forming an opening through which a membranous bag may be introduced, and from which it may be suspended. The electrolyte which I employed in all the following experiments was the same as that which I employ in the constant battery, viz. eight parts of water, by measure, to one part of oil of vitriol in contact with the generating surface, and the same diluted acid, further saturated with sulphate of copper, in contact with the conducting surface :—

The measures of the force to which I had recourse were, in the first instance, a coarse galvanometer consisting of a single needle seven inches in length, with a coil of twenty turns of wire one-twentieth of an inch in diameter. The large scale of the experiments precluded me from employing delicate instruments with astatic needles. Being dissatisfied with this measure, I afterwards employed a Bréguet's thermometer fitted up according to the plan of Professor De la Rive†, which measures the force of a current which passes through it by the differences of its heating power upon the compound spiral of platinum and silver of which it consists, the degrees into which it is divided being directly proportioned to such differences. I had every reason to be satisfied with this instrument; and have no doubt of its accuracy, in the case, at least, of currents of such low tension as those for which alone I have hitherto employed it.

\* *Annals of Electricity, &c.* Vol. I. p. 94.

† *Memoirs de la Soc. de Phys. de Geneve*, 1836, p. 140.

In my first experiment the sphere was charged with the solution of copper, and the membrane with the plain acid, a small sphere of amalgamated zinc, one inch diameter (and exposing a surface therefore of 3.14 square inches) was suspended by means of a well-varnished copper wire in the centre of the latter: the other extremity of the wire was connected with one of the cups of the galvanometer, and the circuit was completed by a wire leading from the other cup to a small mercury cup upon the upper brass hemisphere, placed at a distance of two inches from the tube. The deviation of the needle was  $60^{\circ}$ ; and it remained steady for a long period, during which the experiment was repeated and varied. When, instead of the galvanometer, the circuit was closed with a piece of platinum wire one inch in length and  $\frac{1}{16}$  inch diameter, it continued red hot for a period of five hours. The circuit remained closed for seventeen hours, and the apparatus was then opened and examined. The zinc ball had dropped off the wire, and was reduced to about one half of its original size. The upper hemisphere was found coated with reduced copper, beautifully marked half-way up from the equator with concentric circles of alternate dark and light stripes of pink and red; these were followed by a broad even band of pink, which reached to a circle within  $1\frac{1}{2}$  inch of the aperture, which was composed of the unchanged surface of the brass, and which evidently had not been in contact with the liquid. The lower hemisphere, which had been insulated from the upper by the collar of leather, had no copper precipitated upon it.

The sphere was again put together, and charged as before, with a new zinc ball. The circuit was closed, as in the first experiment, with the galvanometer in contact with the upper hemisphere, and the deviation of the needle was  $60^{\circ}$ .

The connection was then broken with the upper hemisphere, and made with the bottom of the lower hemisphere; the deviation was again  $60^{\circ}$ .

When connexion was made at the same time with both hemispheres, the deviation was the same. If, while in this state, either wire were lifted singly from its connexion, the needle remained perfectly steady.

When the wire from the zinc ball was lifted from the cup, there was a spark upon breaking the circuit; and there was also a spark when the wires from the two hemispheres were disconnected from the circuit together, but not when lifted singly.

Upon leading the conducting wire of the lower hemisphere into direct communication with that of the zinc ball, while the wire of the upper hemisphere was connected as before with the galvanometer, or *vice versa*, the needle only receded to  $40^\circ$ . From this it appeared that, although the whole amount of force originating at the zinc was capable of passing off by means of either hemisphere singly, when both passages were open it distributed itself between the two, notwithstanding the additional resistance in one by the interposition of the galvanometer.

Extra contacts made with different parts of the two hemispheres made no difference in any of the preceding experiments.

The apparatus was now left for eighteen hours with both hemispheres in connexion with one extremity of the galvanometer, and the zinc ball with the other. Upon examination the needle was found still deflected  $25^\circ$ ; and upon agitation of the zinc ball it rose to  $55^\circ$ , but almost immediately declined to  $25^\circ$ . The acid had become nearly saturated with oxide of zinc. The sphere was opened, and both hemispheres exhibited a perfectly even coating of beautiful pink copper, through which the rings of the upper hemisphere were however still discernible.

It will perhaps be advantageous, before I proceed further, to collect into one point of view the principal conclusions which, I conceive, may be drawn from the preceding experiments with the spherical combinations.

1st. The force emanating from the active zinc centre diffused itself over every part of the upper hemisphere, from which there was a good conducting passage for its circulation, to an amount which is measured by  $60^\circ$  of the galvanometer; and which was capable of maintaining an inch of platinum wire  $\frac{1}{160}$  inch diameter at red heat.

2nd. The same amount of force was maintained by either hemisphere indifferently; but when both conducting hemispheres were in metallic communication there was no increase of force. The transfer of half the power from one hemisphere to the other occupied no appreciable interval of time.

3rd. Although the force was not increased, it spread itself equally over the whole sphere, as manifested by the diffusion of the precipitated copper.

4th. When one hemisphere was connected with the zinc centre by a short wire capable of affording circulation to the whole force, and the other hemisphere was connected

by a long wire through the galvanometer with the same centre, the diffusion of the force over the whole sphere was maintained, although the half of it was obliged to overcome the much greater resistance of the longer circuit.

5th. There was no greater accumulation of precipitated copper about the points with which the conducting wires were brought into contact, and towards which the force diffused over the sphere must have converged, than at any other point; proving that the force must have diverged from the centre equally through the electrolyte, and could only have drawn towards the conducting wires in the conducting sphere itself.

I now destroyed the insulation of the two hemispheres by fixing to the lower one a thin ring of brass, which came in contact with the upper when wedged down in its position by the means already described. The sphere, thus in good metallic communication in every part, was charged as before; the precipitated copper having been previously cleaned off by a little nitric acid and rotten-stone. The deviation of the galvanometer was  $55^{\circ}$ , and it was perfectly steady, whatever number of connexions were made with the sphere, or at whatever point the circuit was completed. It made no difference in the amount of force which circulated, whether the contact with the brass was made as near as possible to the zinc conducting wire, or at the point the farthest removed from it.

I next proceeded to ascertain what would be the effect of increasing the surface of the generating metal. For this purpose two amalgamated zinc balls of the same diameter as before were placed in contact upon a varnished wire, and substituted for the single ball in the centre of the sphere: the deviation of the galvanometer only increased  $5^{\circ}$ , rising from  $55^{\circ}$  to  $60^{\circ}$ . Upon replacing the single ball it fell again to  $55^{\circ}$ , and so alternately rose and fell to the same amount upon frequent repetitions of the change. An amalgamated zinc rod six inches in length, and  $\frac{1}{4}$  inch in diameter, was then substituted for the balls, but the deviation did not rise higher than  $60^{\circ}$ . Thus the generating surface was increased from about three square inches to six and 9.4 with very little increased effect.

These experiments were all made with the generating metal placed as nearly as possible in the centre of the conducting sphere: this position was now changed, and sometimes the zinc ball was placed at the bottom of the membrane almost in contact with the sphere; sometimes



it was drawn up nearly to the top ; again it was placed in the centre ; but none of these changes produced any appreciable alteration in the deviation of the needle, the galvanometer constantly indicating  $55^{\circ}$ .

From this I was led to consider (although I am quite aware that the measures may not have been sufficiently accurate to determine the point with precision,) whether the force emanating from the zinc ball might not diffuse itself over the surrounding conducting sphere in obedience to the well-known law of radiant forces of the inverse square of the distance ; since, although the cases may be dissimilar, according to this law an attractive point placed within an hollow sphere of attractive matter remains in equilibrio, whatever its position may be. Or the analogy, perhaps, is stronger of light diffusing itself from a luminous point within the sphere in the same relative positions as those of the generating ball.

This conclusion, if correct, appearing to me to be of great importance, I was desirous of confirming it by repetition and variations of the experiment.

For this purpose I made use of the calorific galvanometer (as it has been named by M. De la Rive,) instead of the magnetic ; and the zinc ball was a little larger than in the preceding experiments, measuring  $1\frac{1}{8}$  inch in diameter, and presenting therefore a surface of 3.94 square inches. The sphere was charged as before, and when the ball was placed in the centre

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The index remained perfectly steady during several repetitions of the experiments ; and when the connection with the brass sphere was changed to opposite points. There can be no doubt that these results indicate the same equality of action as before ; the slight difference in the lower position being, probably owing to accidental circumstances, such as the unequal thickness of the membrane at that point, or the speedy saturation of the acid when the solution cannot fall away from the zinc. Thus it would appear, that in none of these positions was there any virtual approximation of the generating and conducting surfaces. The diffusion of the precipitated copper was, however, very much influenced by the position of the ball ; when near the top or the bottom, it was thrown

down in a compact layer on a segment immediately in its vicinity, and became thinner and thinner over the more remote parts of the sphere. It was only when exactly in the centre that the diffusion was perfectly equal. To observe this effect with the greatest distinctness, the action in each case should not be allowed to continue more than ten or fifteen minutes; for the deposition becomes compact in every part in a longer time, when it is not so easy to judge of the difference of thickness.

My next step was to measure the effects of different portions of the sphere in combination with the zinc ball. I took the lower hemisphere alone of the apparatus and filled it with the solution of copper, and placing a wooden bar across it which supported the membranous bag to carry the acid, I had it in my power to immerse the ball in any required position.

I first placed the same ball as in the last experiment just below the surface exactly in the centre, and the calorific galvanometer indicated  $90^{\circ}$ : upon removing it within half an inch of the bottom it rose to  $115^{\circ}$ .

On another occasion I placed the ball in three different positions upon the surface of the liquid in the hemisphere, namely, in the centre and close to each side: in each position the instrument marked  $86^{\circ}$ . Upon lowering it as close as possible to the bottom it rose to  $100^{\circ}$ . In all these experiments the precipitated copper was diffused over the whole hemisphere.

It is worthy of remark, that the amount of force thus called into action at the surface of the liquid in the hemisphere, is nearly the same as that from the whole charged sphere, or either of its hemispheres.

These results, I conceive, are not in opposition to the law of radiant forces suggested by the experiments with the entire sphere; although I cannot vouch for the comparative measures being absolutely correct or uninfluenced, in some degree, from day to day by extraneous circumstances, and particularly by changes of temperature: nor can it, indeed, be expected that the experimental deductions from a ball of the dimensions which I employed can do more than approximate to the mathematical demonstrations of the relations of an active point within an attractive sphere.

An accidental circumstance next furnished me with an interesting variation in the combination of the whole sphere. My intention had been to fill it as before, and

repeat the experiments; but I afterwards found that the liquid only reached to within  $45^{\circ}$  of the vortex. The zinc ball which I made use of was  $\frac{1}{4}$  inch in diameter, and the membrane was full of the dilute acid. The experiments were made in three different positions of the ball, and with three variations of the wire connecting the circuit with the brass sphere.

The first connection was made near the top at a point which was not within the contact of the included liquid; the results were

Caloric Galv.

At the top .....	45 <sup>o</sup>
Centre .....	55
Bottom.....	65

Connexion with the bottom of sphere.

Ball at the top.....	45 <sup>o</sup>
Ball at the centre .....	70
Ball at the bottom .....	77

Connexion with the sphere both at the top and bottom.

Ball at the top.....	45 <sup>o</sup>
Ball at the centre .....	55
Ball at the bottom .....	65

The two last series may be taken to be identical, but I am a loss to explain the difference of the first series from those two. It is probable that the zinc ball, though wholly immersed in the acid in the membrane, was not below the surface of the exterior solution in the sphere. The results confirm in a general way the conclusions which have been previously suggested.

There was yet another combination which I thought it desirable to try; namely, with the generating ball placed within the charged sphere with its two hemispheres insulated. The zinc ball which I employed for this experiment was one inch diameter, and I tried it first with the standard acid with which all the other experiments had been made, and repeated it with the same acid diluted with an equal bulk of water, in order that I might ascertain whether the law of action was influenced at all by the amount of force put into circulation. The results were perfectly consistent with each other.

	Standard Acid.	Dilute Acid.
The zinc ball placed near the bottom, connexion with top insulated hemisphere ...	52	42
Connected with bottom .....	59	50
The zinc ball placed near the top, connexion with bottom insulated hemisphere	51	41
Connected with top .....	59	50
Both hemispheres connected .....	59	

In these experiments, that hemisphere alone with which the connexion had been made could have influenced the results as a conducting surface.

A question now occurred to me of extreme interest: admitting that the force generated in the preceding combinations where it was limited in its diffusion by the concave surfaces of spherical forms, follows in its action the law of radiant forces, we know that it is not a *simple* radiant force like that of gravity, but a molecular force propagated from particle to particle, and possibly modified by other forces with which the same particles may be endowed. The law of radiation, if established, may, therefore, be the result of the concurrence of more than one force, limited by the spherical combinations with which we have been dealing. The physical law, in ordinary cases, supposes both the sphere and the point with to consist of similar attractive matter; and were the force which we are now investigating of the same nature, it would make no difference whether the sphere consisted of merely conducting metal and a small interior generating ball, or whether the sphere consisted of generating metal and the ball were an inactive conductor,—inactive I mean with regard to the electrolyte. What experimental results, therefore, would the latter combination afford?

I caused two hemispheres of zinc to be made of exactly the same form and dimensions as those of copper, and fitted together in the same way; and the interior surfaces of these were thoroughly amalgamated with mercury. When wedged together by their flanges and ring of leather, metallic contact was preserved between the two by a small interior ring of amalgamated zinc. The sphere was charged with dilute acid, and the interior membranous bag with the acid solution of copper. A copper ball of one inch diameter fixed to the end of a varnished wire was

then immersed in the latter, and a circuit formed by contact with different parts of the zinc sphere with the intervention of the calorific galvanometer.

The indications of the instrument were as follow :

Ball at the top.....	36 <sup>2</sup>
Ball at the centre .....	36
Ball at the bottom .....	36
Again at the top .....	35

The index was steady for a short time, but began slowly to decline in each instance for about 10°, Upon agitation of the ball in the solution it always rose to its previous amount.

Upon repeating the experiment and keeping the ball always agitated, the following results were obtained :—

Ball at the top.....	40 <sup>0</sup>
Ball at the centre .....	42
Ball at the bottom .....	42

Another repetition gave

Ball at the top.....	45 <sup>0</sup>
Ball at the centre .....	46
Ball at the bottom .....	44

The index always fell when the ball was not agitated, and the decline may probably be ascribed to a change in the saturation of the liquid in immediate contact with the ball, which agitation prevented by keeping the solution in an uniform state.

I now separated the hemisphere and experimented with the lower one alone. The results were as follow :—

Ball at the top.....	35 <sup>0</sup>
Ball at the bottom .....	80
Ball at the top.....	35
Ball half-way between...	60

When the ball was placed at the centre of the surface and at the sides almost in contact with the hemisphere, the index rose to 36° in all the three positions.

Hence it appears that the same law was maintained ; although the force which circulated was reduced to one half of the amount of that from the first combination.



Before I venture to offer two or three remarks upon this difference, I shall proceed to lay before you the results of some experiments upon combinations of generating and conducting surfaces of other forms, commencing with the cylindrical as approaching in simplicity to the spherical, and for the purpose of connecting my observations with that form which I have found most practically advantageous in the construction of my battery.

I took one of the cells of the small battery, six inches in height and eleven inches in circumference, charged in the usual way, and found that the single zinc ball of one inch diameter, produced with it a deflection of the magnetic galvanometer of  $55^{\circ}$ . Two similar balls only increased the deflection to  $60^{\circ}$ , and its own rod, six inches in length and  $\frac{5}{8}$  inch diameter, did not increase the effect. These were exactly the same amounts as were produced by the same generating surfaces in the brass sphere.

An amalgamated zinc cell of exactly the same dimensions, charged with the diluted acid and solution of copper in the membrane, with a copper ball one inch diameter, deflected the galvanic needle  $45^{\circ}$ .

With the calorific galvanometer it moved the index to  $25^{\circ}$ .

Being desirous of ascertaining whether the same difference between the reversed combinations would exist in a series as in single circuits, I compared together three copper cylinders with three zinc rods, and three zinc rods, and three zinc cylinders with copper rods, by means of a voltameter. The former produced in  $\frac{1}{4}$  hour 3.6 cubic inches of gas. The latter produced in  $\frac{1}{4}$  hour 1.75 cubic inches: indicating, as before, that when the generating surface constituted the circumference of the arrangement, the force was only the half of that which was evolved when it formed the centre.

In the copper cylinder it made no difference whether the zinc ball or rod were placed in the centre, or nearly in contact with the side.

I next took an oval copper plate, the diameters of which were  $13\frac{1}{2}$  inches by 10 inches, and soldered a copper wire at one extremity of the longest diameter, in a perpendicular position, and placing it in an earthen pan, covered it with a depth of  $4\frac{1}{2}$  inches of the acid solution of copper. Over its centre I suspended, by means of a cross bar, a bag of membrane filled with the dilute acid; and nearly at the bottom of this I placed an amalgamated zinc ball, connected with a varnished copper wire. I then formed a circuit by

means of the magnetic galvanometer, and the needle was deflected  $55^{\circ}$ . It remained perfectly steady for half an hour, when the plate was taken out and examined. It was found covered with fresh precipitated copper, the coat being a little thicker at the centre, and becoming thinner by almost insensible gradations towards the edge. At one point it had begun to turn round the edge and to diffuse itself on the other side.

The zinc ball was next placed just below the surface of the liquid in the pan, so that its least distance from the conducting plate was the same as when placed in the centre of the sphere. Upon completing the circuit the needle was deflected  $45^{\circ}$ .

The zinc ball was now drawn up in the tube so as to remain immersed in the acid, but two inches above the level of the solution; the needle was still deflected  $35^{\circ}$ . Upon again replacing it below the level it returned to  $45^{\circ}$ , and fell to the same amount upon once more drawing it up. In this case the lateral diffusion of the efficient force must have been prevented for the first two inches of its course; the mode of its after propagation was the subject of my next inquiry. While the apparatus was in this position, one zinc rod, and afterwards two, connected together, were immersed into the acid, so as to extend through the whole depth of the solution, without deflecting the needle more than  $55^{\circ}$ .

In these experiments the stem of the copper wire which was soldered to the copper plate was covered with precipitated copper as well as the plate. In subsequent experiments it was protected by varnish.

In order effectually to cut off the lateral radiation from the zinc ball, I placed it in a glass tube, six inches long; and over the lower aperture, which was  $1\frac{1}{2}$  inch diameter, I tied a piece of membrane: the tube was then filled with dilute acid and plunged into the solution of copper contained in the brass hemisphere, against the bottom of which it rested. The ball was supported within half an inch of the diaphragm. Upon closing the circuit by means of the galvanometer, the needle indicated  $40^{\circ}$ . At the expiration of five minutes the solution was drawn off and the hemisphere examined: there was found a beautiful well-defined circle of pink copper, two inches diameter, surrounded by a halo of darker colour, evidently of fresh precipitated copper of less thickness, but not extending over more than a fourth part of the surface. From this

experiment it was evident that the force had diverged from the aperture of the glass tube, as from a centre, after it had entered the solution; the circle of pink copper being of a diameter half an inch greater than the aperture, and the fainter halo extending some inches around.

I now moved the tube into such a position, that, the zinc ball, remaining where it was in the tube, might be just below the level of the solution in the hemisphere; and now, notwithstanding the greater distance at which it was placed from the conducting surface, the needle rose to  $45^{\circ}$ , and the precipitated copper made its appearance over the whole hemisphere.

While the zinc ball was in its last position, the glass tube itself was pressed down till it again rested upon the bottom of the hemisphere; all lateral diffusion was thus cut off for a distance of  $4\frac{1}{2}$  inches, through which the force was propagated, and it could only spread after it emerged from the tube; the galvanometer indicated  $30^{\circ}$ . When the glass screen, as it may be called, was again drawn up, the needle returned to  $45^{\circ}$ .

Similar experiments were often repeated with the substitution of large flat plates for the hemisphere. When the zinc ball was thus confined in a tube with a diaphragm, and placed within an inch of the plate, the precipitation always commenced with a circle a little larger than the aperture, and gradually extended itself, so that after some hours action it formed a circle of four or five inches diameter, and sometimes turned the edge, and made its appearance on the under surface.

Being desirous of ascertaining to what amount the under surface of a plate thus immersed in the electrolyte would affect the action, I covered the upper surface of the oval copper plate with lac varnish, and replaced it in its pan; I then placed the zinc ball in the tube within half an inch of the diaphragm, and plunged it just below the surface of the solution; upon completing the circuit the needle indicated  $50^{\circ}$ . In ten minutes time I examined the plate, and found the under side covered with beautiful pink copper, with the exception of an irregular oval space, whose diameters were about  $4\frac{1}{2}$  inches by 4. The precipitation had evidently begun upon the edge, where it was thickest.

The plate was returned to its position, and the galvanometer again indicated  $50^{\circ}$ . When a double ball was substituted for the single it rose to  $55^{\circ}$ . It was again

examined after an hour's action, and on the under side presented an appearance of a border of compact pink copper, varying from  $1\frac{1}{2}$  inch to  $\frac{7}{8}$  inch in width, but not wider at the sides than the ends; and the remainder was covered with precipitated copper of a darker red colour, into which the former graduated. On the upper varnished side there were a number of little rosettes of copper, which were loose, giving it a gritty feel, and which evidently proceeded from a number of minute points to which the varnish had not adhered. Fig. 1. (Plate I.) may convey an idea of the appearance of the precipitated copper. Both surfaces of the copper plate having been cleaned, it was replaced in the solution, and the double zinc ball in the membrane, instead of being placed over the centre, was suspended over that end of the oval which was farthest removed from the conducting wire. The galvanometer indicated  $48^\circ$ , and remained perfectly steady for an hour, during which the action was continued. At the expiration of this time the plate was examined, and the pink copper was found deposited only at the extremity under the ball, forming a segment of a circle, of which the ball was the centre, but still not sharply defined, but imperceptibly shading off by a darker colour to the unchanged surface at the other extremity.

Here the efficiency of the generating ball was evidently impeded by the deficiency of the conducting plate on one side. The radiation could only take place towards the other side; and although the conductor was of great extent, the increasing distance of its several points caused it to act at a disadvantage.

After these experiments, the plate was cleaned and replaced in the solution; and in order to determine the action of more than one generating point, at a distance from one another upon the same conducting plate, I first placed a single zinc ball in a membrane close to the conducting wire of the plate, and found the deviation of the galvanometer  $42^\circ.5$ . I then broke this connexion, and placed a double ball of zinc over the farthest extremity of the oval; the galvanometer again marked  $42^\circ.5$ . When the circuit was completed with both together, the galvanometer only rose to  $45^\circ$ . Upon making or breaking contact with either ball singly, while the other remained in circuit, the needle only varied  $2^\circ$  or  $3^\circ$ . When the positions of the double and single ball were changed, the difference upon breaking the contact with it was  $5^\circ$ . After

four hours action, the plate was examined, and the pink copper was found deposited in two segments of circles, of which the balls had been the respective centres; and the remainder of the plate was covered with a precipitation of a redder colour. (fig. 2.) This thinner precipitation gave no indications of an increased substance as evidence of additional action at the points where the effects spreading from the two centres must be conceived to have been superimposed.

The plate having been once more cleaned, the double ball was placed nearly over its centre, and when connected with the galvanometer in circuit it affected it  $55^{\circ}$ . The single ball placed also nearly over the centre, but about two inches removed from the position of the double ball, affected it singly  $45^{\circ}$ . When the connexion was made with both the deflection increased to  $57^{\circ}$ . After four hours action with both the balls, a beautiful well-defined oval of pink copper precipitate was found immediately under the two balls, of the diameter of 4 inches by  $3\frac{1}{2}$  nearly, beyond which there was a general diffusion of darker red precipitate (fig. 3). The surface of this oval was perfectly smooth and compact, and presented no appearance of greater thickness or inequality at the centre than at any other part. The two balls seem to have acted as one oblong piece of generating metal would have done; and, upon the hypothesis of a radiant force from each, no increase was perceived at the points upon which the double set of rays might have been supposed to impinge.

I was desirous to repeat once more the experiments with the opposite sides of the conducting plate, and to connect them more closely with some of the previous series, by measuring the effects upon the calorific galvanometer, and by varying the form of the plate. For this purpose, I took a circular plate of copper of the same diameter as the sphere, and immersed it in the copper solution. A zinc ball of  $1\frac{1}{2}$  inch diameter, placed in a membrane of acid over the centre, gave the following results:—

	First Series.	Second Series.
Ball at top.....	79	82
Ball at middle .....	89	92
Ball at bottom .....	103	105

After these experiments, the copper was found diffused over both sides of the plate, but did not extend to the centre of the under side.

The upper surface of the plate having been thoroughly covered with lac varnish, the experiment was repeated with the following results :—

Ball at top .....	69
Ball at middle .....	75
Ball at bottom .....	70

The precipitated copper was here found deposited upon the under side in a ring about two inches in breadth from the edge, and there was very little in the centre of the plate, and none upon the upper surface.

I finally covered the under surface with varnish, leaving the upper exposed, with the following results :—

Ball at top .....	73
Ball at middle .....	83
Ball at bottom .....	93

Hence it appears, that the under surface, which by itself is capable of sustaining an action from the ball in the centre of the solution nearly as great as the upper surface, when combined with the latter, adds no more than 10°, or about one-eighth to its efficiency. It appears also that, whereas with the upper surface, the action increases in some inverse ratio of the distance, of the generating from the conducting surface, with the under surface, there is a maximum point, on both sides of which it decreases. This point is doubtless dependent upon the angle at which the force, which radiates (as it were) from the ball, meets the edge of the plate.

Wishing now to draw the preceding experiments into closer comparison with similar ones which had hitherto, generally, been made in a different form, I had a square glass cell constructed, measuring  $3\frac{1}{2}$  inches wide and four deep. This was cut in two in the middle, and the edges having been ground, admitted of being clamped together, with the interposition of a piece of bladder. When thus put together it formed a cell divided into two by a diaphragm of membrane in the liquid, on either side of which different generating and conducting plates might be immersed. The forms which I selected were copper and zinc plates  $3\frac{1}{2}$  inches square, and copper and zinc balls of one inch diameter: the results obtained by different combinations of these, at different distances apart, measured by the calorific galvanometer, are contained in the following Table. The electrolyte which was employed was, in one

series, the standard acid on both sides of the diaphragm, and in the other the same acid in contact with the zinc, and the solution of copper with the copper. In the near distance the surfaces were about half an inch apart; in the farther, three inches; and the balls were always placed opposite to the centre of the associated plate.

	Equal Plates.		Copper Plate and Zinc Ball.		Zinc Plate and Copper Ball.		Equal Balls.	
	Close.	Distant.	Close.	Distant.	Close.	Distant.	Close.	Distant.
Acid alone . . .	13	13	17	18	8	8	0	8
Acid & Sol. Cop.	75	51	67	53	32	30	26	24

The precipitated copper in all these cases had been pretty evenly diffused over the near surface of the plates, and was to be traced to all parts of the opposite surface, but more upon the edges than towards the centres.

Upon this Table we may remark :—

1st. That the energy of the force was about sextupled by the absorption of the hydrogen upon the conducting surface, except in the case of the equal plates, when it was more than quadrupled.

2nd. That the effect of distance was much more decided in the instances where the amount of the circulating force was greater, than in the contrary cases.

3rd. That the amount of force put into circulation from a large surface of zinc towards a central ball of copper was, as in former instances of similar combinations, about one half of that from the reverse arrangement.

4th. That a ball of zinc exposing a surface of 3.14 square inches placed over the centre of a plate of copper exposing on its two sides a surface of twenty-eight square inches, sustained an action of nearly the same amount as a plate of zinc of the same dimensions as the copper, placed at the same distances.

This result, as well as the small effect produced in preceding experiments by substituting two equal balls, or a rod, for one ball of zinc, may, upon the supposition of a force of the nature of a radiant force, be probably explained by the interference of the rays at points where their directions cross each other.

Let A B C D, fig. 4, represent the sphere, and *a* and *b* two active points within; the force radiates from *a* to *c* and *A* and *d*, and from *b* to *e* and *f* and *C* without interruption.

But the rays proceeding from *a* to *g* and from *b* to *g*, encounter one another, and the force would appear to be directed in the diagonal of the two, or from *g* to *B*, and in a direction parallel to *a c* and *b c*.

A rod may obviously be considered as a succession of such balls or radiant points, and hence the force would be propagated in a direction at right angles to its axis towards a circumscribing sphere or cylinder.

Let *abcd*, fig. 5, represent the radiant points, then will the rays *a g* and *b g* interfere at *g* and pass on to *i*, *b f* and *c f* to *B*, and *c e*, *d e* to *l*. Or the same letters may represent radiant points in a plate *A C*, the rays of which will thus pass in a parallel direction to the opposite points of a conducting plate *D E*.

Without attaching, however, any importance to the geometrical diagrams, I would merely suggest that the resultants of all the radiant points acting from *a* to *d* towards *D E* may be parallel, and that such an hypothesis would account for the phenomena. The demonstration of this, if possible, would go far beyond my power in mathematical science.

The rays thus supposed to pass between two plates become parallel, and hence the decrease of the force will be directly as the distance, as Mr. SNOW HARRIS found it by direct experiment.

Now nearly the whole of the preceding experiments, except those of the zinc sphere, had been made before I had the pleasure of reading the Eleventh of your Series of Experimental Researches on Electric Induction;\* and I had been led to the supposition, which I believe I mentioned to you, that the force which is developed by voltaic combinations might be subject to the law of radiant forces; but I had been utterly at a loss to understand how, upon this hypothesis, it could extend its influence to the side of a plate opposite to that to which it was directed in right lines; how, in short, (to make use of a term which you have happily employed to describe, what I now believe to be, a perfectly analogous phenomenon), it could "*turn a corner*." Since the perusal of that paper, however, every thing seems to me to fall in so naturally with the general views which you have therein explained, that I almost feel as if I were intruding upon ground which is properly your own in venturing to apply the laws which you have estab-

\* Philosophical Transactions, 1838, p. 1.



lished of the "Essential and Fundamental Principle of Induction"\* to the explanation of some of the foregoing results. Supposing my views to be correct, you must have been led to them in the natural course of your investigations; and nothing in my own opinion could justify my interference in a work which must have been more completely performed by you, but the circumstance that I was led to it by the obvious direction of my own previous inquiries into voltaic combinations.

The direction of the force through an electrolyte placed between a generating and conducting surface of two metals, may, I conceive, be expressed in the very words which you have employed to describe that of the *direct inductive force* in statical electricity.

"It may be conceived to be exerted in lines between the two limiting conducting surfaces, and is accompanied by a lateral or traverse force equivalent to a dilation or repulsion of these representative lines; or the attractive force which exists amongst the particles of the electrolyte (dielectric) in the direction of the current (induction) is accompanied by a repulsive or diverging force in the transverse direction†."

The proof of this is exactly of the same nature as that which you have brought forward in the parallel instance of induction, namely, the turning round the corner of a plate; and I cannot but advert to the complete analogy of the case in which brought your carrier ball near to the middle of a flat disc of metal placed upon an excited shell-lac cylinder when no charge was communicated, although one was obtained at the edge of the disc; and that of the deposition of a ring of precipitated copper round the edge of the under surface of a brass plate while the centre was free from it (fig. 1.).

This "lateral tension of the lines of force on one another" is quite consistent with their *divergence* from an active centre; may it not even be considered as the cause of their radiation; it is most particularly evinced by the results of those experiments, in which the immediate divergence of the force from the active centre was prevented, by placing the latter in a glass tube, or by drawing it up above the general level of the surrounding electrolyte. In these instances the first impulse must have been propagated in a perpendicular direction; but the instant it

\* Annals of Electricity, Vol. 5. † Ibid.

was at liberty to influence the general mass, the molecules of the latter were thrown into the polarized state, and the direction of the force opened out as from a centre.

On the other hand, the same "repulsive force in a transverse direction" must be opposed to the *convergence* of the lines from an active sphere towards an interior conducting point, when the force is not stationary but current: may not this opposition account for the reduced action of a sphere of zinc upon a ball of copper? The difference of the *statical induction* and the *current induction* is, that in the former the force is not progressive, while in the latter it is in a state of perpetual flux; the state of polarity, however, and of tension, is maintained in both.

The transfer of the elements of the electrolyte in opposite directions under that peculiar molecular arrangement or polarity, "which is the first step in all electrolyzation," is quite compatible with their unequal distribution upon the limiting conducting surfaces, according to the varying relations of their dimensions and distances, as was evidenced by the unequal precipitation of the reduced copper in several of the preceding instances; but no correspondent inequality of the force can exist upon the surface of the conductors themselves, upon all parts of which it can instantaneously distribute itself with comparative facility.

The principal circumstance which might be supposed to limit the power of an active point within a conducting sphere in any given electrolyte, is the resistance of that electrolyte, which increases in a certain ratio to its depth or thickness; and this thickness may be considered virtually the same, wherever the included point may be placed, but to increase with the diameter of the sphere. It is also the same, and consequently the resistance is also the same, when placed anywhere within the plane which divides the sphere into two hemispheres. But in an insulated hemisphere, the approximation of the active point to the lower surface virtually decreases the thickness of the electrolyte through which its action has to be propagated, by increasing the extent of surface which cuts the divergent lines of force; and consequently the force increases. In this respect the action of a point upon a plate may be considered the same as upon an indefinitely large hemisphere, towards which as the point approaches the force increases.

It appears to me that practical consequences of some importance flow from the preceding conclusions, upon which I may be tempted to address you again at no distant period.

I remain, my dear Faraday,  
Ever faithfully yours,  
J. F. DANIELL.

*King's College, January 13, 1838.*

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*Contributions to Electricity and Magnetism, No IV. On Electro-dynamic Induction. (Continued.)* By JOSEPH HENRY, L.L.D., Professor of Natural Philosophy in the College of New Jersey, Princeton.\*

*Introduction.*

1. IN the course of my last paper, it was stated that the investigations which it detailed were not as complete in some parts as I could wish, and that I hoped to develop them more fully in another communication. After considerable delay, occasioned by alterations in the rooms of the physical department of the college, I was enabled to resume my researches, and since then I have been so fortunate as to discover a series of new facts belonging to different parts of the general subject of my contributions. These I have announced to the Society at different times, as they were discovered; and I now purpose to select from the whole such portions as relate particularly to the principle subject of last paper, namely, the induction at the beginning and ending of a galvanic current, and to present to them as a continuation, and, in a measure, as the completion, of this part of my researches. The other results of my labours in this line will be arranged for publication as my duties will permit me to give them a more careful examination.

2. In the course of the experiments I am about to describe, I have had occasion to repeat and vary those given in my last paper; and I am happy to be able to state, in reference to the results, that, except in some minor particulars, which will be mentioned in the course of this paper, I have found no cause to desire a change in the accounts before published. My views, however, of

\* From the transactions of the American Philosophical Society, vol viii, read June 19, 1840.

the connexion of the phenomena have been considerably modified, and I think rendered much more definite by the additional light which the new facts have afforded.

3. The principal articles of apparatus used in these experiments are nearly the same as those described in my paper, namely, several flat coils and a number of long wire helices (III. 6, 7, 8.\*) I have, however, added to these a constant battery, on Professor Daniel's plan, the performance of which has fully answered my expectations, and confirmed the accounts given of this form of the instrument by its author. It consists of thirty elements formed of as many copper cylinders, open at the bottom, each five inches and a half in height, three inches and a half in diameter, and placed in earthen cups. A zinc rod is suspended in each of these, of the same length as the cylinders, and about one inch diameter. The several elements are connected by a thick copper wire, soldered to the copper cylinder of one element, and dipping into a cup of mercury on the zinc of the next. The copper and zinc, as usual, are separated by a membrane, on both sides of which is placed a solution of one part of the sulphuric acid in ten parts of water; and to this is added, on the side next the copper, as much sulphate of copper as will saturate the solution. The battery was sometimes used as a single series, with all its elements placed consecutively, and at others in two or three series, arranged collaterally, so as to vary the quantity and intensity of the electricity, as the occasion might require.

4. The galvanometers mentioned in this paper, and referred to in the last, are of two kinds; one, which is used with a helix to indicate the action of an induced current of intensity, consists of about five hundred turns of fine copper wire, covered with cotton thread, and more effectually insulated by steeping the instrument in melted cement, which was drawn into the space between the spires by capillary attraction. The other galvanometer is formed of about forty turns of a shorter and thicker wire, and is always used to indicate an induced current, of considerable quantity, but of feeble intensity. The needle of both these instruments is suspended by a single fibre of raw silk.

5. I should always state, that in all cases where a magnetizing spiral is mentioned in connexion with a helix, the

\* When the numerals II. or III. are included in the parentheses, reference is made to the corresponding Nos. of my contribution, *Annals of Electricity, &c.*, vol. 4, p. 181.

article is formed of a long, fine wire, making about one hundred turns around the axis of a hollow piece of straw, of about two inches and a half long: also the spiral mentioned in connexion with a coil is formed of a short wire, which makes about twenty turns around a similar piece of straw. The reason of the use of the two instruments in these two cases is the same as that for the galvanometers, under similar circumstances, namely, the helix gives a current of intensity, but of small quantity, while the coil produces one of considerable quantity, but of feeble intensity.

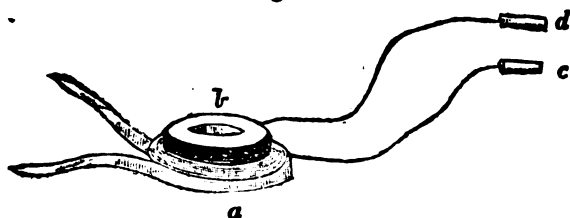
**SECTION I.—***On the Induction produced at the moment of the beginning of a Galvanic Current, &c.*

6. It will be recollected that the arrangement of apparatus employed in my last series of experiments gave a powerful induction at the moment of breaking the galvanic circuit, but the effect at making the same was so feeble as scarcely to be perceptible. I was unable in any case to get indications of currents of the third or fourth orders from the beginning induction, and its action was therefore supposed to be so feeble as not materially to affect the results obtained.

7. Subsequent reflection, however, led me to conclude, that in order to complete this part of my investigations, a more careful study of the induction at the beginning of the current would be desirable; and accordingly, on resuming the experiments, my attention was first directed to the discovery of some means by which the intensity of this induction might be increased. After some preliminary experiments, it appeared probable that the desired result could be obtained by using a compound galvanic battery, instead of the single one before employed. In reference to this conjecture the constant battery before mentioned (3.) was constructed, and a series of experiments instituted with it, the results of which agreed with my anticipation.

8. In the first experiment, coil No. 2, which it will be remembered (III. 7.) consists of a copper riband of about sixty feet long, and coiled on itself like the main spring of a watch, was connected with the compound battery, and helix No. 1 (III. 8.), formed of one thousand six hundred and sixty yards of fine copper wire, was placed on the coil to receive the induction, as is shown in Figure 1, which is again inserted in the next page for the convenience of the reader.

Fig. 1.



*a* represents No. 1, *b* helix No. 1, and *c*, *d*, handles for receiving the shock.

This arrangement being made, currents of increasing intensity were passed through the coil by constantly retaining one of its ends in the cup of mercury forming one extremity of the battery, and successively plunging the other end into one of the cups which served to form the connexions of the several elements of the battery. With the current from one element, the shock of breaking the circuit was quite severe; but at making the same it was very feeble, and could be perceived in the fingers only or through the tongue. With two elements in the circuit, the shock at beginning was slightly increased; with three elements the increase was more decided, while the shock at breaking the circuit remained nearly of the same intensity as at first, or was comparatively but little increased. When the number of elements was increased to *ten*, the shock at making contact was found fully equal to that at breaking, and by employing a still greater number, the former was decidedly greater than the latter; the difference continually increasing until all the thirty elements were introduced into the circuit.

9. In my last paper, a few experiments are mentioned as being made with a compound battery of Cruickshank's construction; but from the smallness of the plates of this, and the rapidity with which its power declined, I was led into the error of supposing that the induction at the ending of the current, in the case of a short coil, was diminished by increasing the intensity of the battery (see paragraph 19. of No. III.); but by employing the more perfect instrument of Professor Daniell in the arrangement of the last experiment, I am enabled to correct this error, and to state that the induction at the ending remains nearly the same when the intensity of the battery is increased. If the induction depends in any degree on the quantity of current electricity in the conductor, then a

slight increase in the induction should take place; since, according to theory, the current is somewhat increased in quantity in the case of a long coil, by the increase of the intensity of the battery. Although very little, if any, difference could be observed in the intensity of the shock from the secondary current, yet the snap and deflagration of the mercury appeared to be greater from the primary current when *ten* elements of the battery were included in the circuit, than with a single one. The other results which are mentioned in my last paper, in reference to the compound battery, are, I believe, correctly given.

10. The intensities of the different shocks in the foregoing experiments were compared by gradually raising the helix from the coil (see Fig. 1), until, on account of the distance of the conductors, the shock in one case would be so much reduced as to be scarcely perceptible through the fingers or the tongue, while the shock from another arrangement, but with the same distance of the conductors, would be evident, perhaps, in the hands. The same method was generally employed in the experiments in which shocks are mentioned as being compared, in other parts of this paper.

11. Experiments were next made to determine the influence of a variation in the length of the coil, the intensity of the battery remaining the same. For this purpose, the battery consisting of a single element, and the arrangement of the apparatus as represented in Fig. 1, the coil was diminished in length from sixty feet to forty-five, then to thirty, and so on. With the first-mentioned length the shock, at making contact with the battery, was, of course, very feeble, and could be felt only in the tongue; with the next shorter length it was more perceptible, and increased in intensity with each diminution of the coil, until a length of about fifteen feet appeared to give a maximum result.

12. The diminution of the intensity of the shock in the last experiment, after the length of the coil was diminished below fifteen feet, was due to the diminution of the number of spires of the coil, each of which, by acting on the helix, tends to increase the intensity of the secondary current, unless the combined length of the whole is too great for the intensity of the battery. That this is the fact is shown by the following experiment: the helix was placed on a single spire or turn of the coil, and the length of the other part of the copper riband, which did not act on the

helix, was continually shortened, until the whole of it was excluded from the circuit; in this case the intensity of the shock at the beginning was constantly increased. We may therefore state generally, that, in the beginning of the battery current, the induction of a unit of its length is increased by every diminution of the length of the conductor.

13. In the experiment given in paragraph 11, the intensity of the shock at the *ending* of the battery current diminishes with each diminution of the length of the coil; and this is also due to the decrease of the number of the spires of the coil, as is evident from an experiment similar to the last, in which the helix was placed on a coil consisting of only two turns or spires of copper riband; the shock at the ending, with this arrangement, was comparatively feeble, but could be felt in the hands. Different lengths of coil No. 2, were now introduced into the same circuit, but not so as to act on the helix; but although these were varied from four or five feet to the whole length of the coil (sixty feet) not the least difference in the intensity of the shock could be perceived. We have, therefore, the remarkable result, that the intensity of the ending induction of each unit of length of the battery current is not materially altered, at least within certain limits, by changing the length of the whole conductor. From this we would infer that the shock depends more on the intensity of the action than on the quantity of the current, since we know that the latter is diminished in a given unit of the conductor by increasing the length of the whole.

14. We have seen (8.) that, with a circuit composed of ten elements of the compound battery and the coil No. 2, the shock, at the beginning of the current, was fully equal to that at the ending. It was, however, found, that if, in this case, the length of the coil was increased, this shock was diminished; and we may state, as an inference from several experiments, that however great may be the intensity of the electricity from the battery, the shock at the beginning may be so reduced, by a sufficient increase of the length of the primary circuit, as to be scarcely perceptible.

15. It was also found, that when the thickness of the coil was increased, the length and intensity of the circuit remaining the same, the shock at the beginning of the battery current was somewhat increased. This result was produced by using a double coil; the electricity was made to pass through one strand, and immediately afterwards



through both: the shock from the helix in the latter case was apparently the greater.

16. By the foregoing results we are evidently furnished with two methods of increasing, at pleasure, the intensity of the induction at the beginning of a battery current; the one consisting in increasing the intensity of the source of the electricity, and the other in diminishing the resistance to conduction of the circuit while the intensity remains the same.

17. The explanation of the effects which we have given, relative to the induction at the beginning, is apparently not difficult. The resistance to conduction in the case of a long conductor and a battery of a single element is so great, that the full development of the primary current may be supposed not to take place with sufficient rapidity to produce the instantaneous action on which the shock from the secondary current would seem to depend. But when a battery of a number of elements is employed, the poles of this, previous to the moment of completing the circuit, are in a state of electrical tension; and therefore the discharge through the conductor may be supposed to be more sudden, and hence an induction of more intensity is produced.

18. That the shock at both making and breaking the circuit in some way depends on the rapidity of formation and diminution of the current, is shown by the following experiment, in which the tension just mentioned does not take place, and in which, also, the current appears to diminish more slowly. The two ends of the coil were placed in the two cups which formed the poles of the battery, and permanently retained there during the experiment; also, at the distance of about six inches from, say the right hand end of the coil, a loop was made in the riband, which could be plunged into the cup containing the left hand end. With this arrangement, and while only the two extreme ends of the coil were in connexion with the cups of mercury, of course the current passed through the entire length of the riband of the coil; but by plunging the loop into the left hand cup, the whole length of the coil, except the six inches before mentioned, was excluded from the battery circuit. And again, when the loop was lifted out of the cup, the whole length included. In this way the current in the coil could be suddenly formed and interrupted, while the poles of the battery were continually joined by a conductor; but no shock with either single or

a compound battery could be obtained by this method of operation.

19. The feebleness of the shock at the beginning of the current, with a single battery and a long coil, is not entirely owing to the cause we have stated, (17.) namely, the resistance to conduction offered by the long conductor, but also depends, in a considerable degree, if not principally, on the adverse influence of the secondary current, induced in the primary conductor itself, as is shown by the result of the following experiment. Helix No. 1. was placed on a coil consisting of only three spires or turns of copper riband; with this, the shock, both at making and breaking the circuit with a single battery, could be felt in the hands. A compound coil was then formed of the copper ribands of coils No. 3. and 4. rolled together so that the several spires of the two alternated with each other, and when this was introduced into the circuit so as not to act on the helix by its induction, and the battery current passed through, for example, coil No. 3, the shock at making contact with the pole of the battery was so much reduced as to be imperceptible in the hands, while the shock at breaking the contact was about the same as before this addition was made to the length of the circuit. The ends of coil No. 4, were now joined so as to produce a closed circuit, the induced current in which would neutralize the secondary current in the battery conductor itself; and now the shock at making the contact was nearly as powerful as in the case where the short conductor alone formed the circuit with the battery. Hence, the principal cause of the feebleness of the effect at the beginning of the battery current, is the adverse action on the helix of the secondary current produced in the conductor of the battery circuit itself. The shock at the breaking of the circuit, in this experiment, did not appear affected by joining or separating the ends of coil No. 4.

20. Having investigated the conditions on which the inductive action at the beginning of a battery current depends, experiments were next instituted to determine the nature of the effects produced by this induction: and first the coils were arranged in the manner described in my last paper (III. 79.), for producing currents of the different orders. The result with this arrangement was similar to that which I have described in reference to the ending induction, namely, currents of the third, fourth, and fifth orders were readily obtained.

21. Also, when an arrangement of apparatus was made similar to that described in paragraph 87 of my last paper, it was found that a current of intensity could be induced from one of quantity and the converse.

22. Likewise, the same screening or rather neutralizing effect was produced, when a plate of metal was interposed between two consecutive conductors of the series of currents, as was described (III. Section IV.) in reference to the ending induction. In short, the series of induced currents produced at the beginning of the primary current appeared to possess all the properties belonging to those of the induction at the ending of the same current.

23. I may mention, in this place, that I have found, in the course of these experiments, that the neutralizing power of a plate of metal depends, in some measure, on its superficial extent. Thus a broad plate which extends, in every direction, beyond the helix and coil, produces a more perfect screening than one of the same metal and of the same thickness, but of a diameter only a little greater than that of the coil.

24. The next step in the investigation was to determine the direction of the currents of the different orders produced by the beginning induction, and for this purpose the magnetizing spirals (5.) were used, and the results obtained by these verified by the indications of the galvanometer. It should be stated here, as a fact which was afterwards found of some importance, that although the needle of the galvanometer was powerfully deflected when the instrument was placed in the circuit of the secondary current, yet a very feeble effect was produced on it by the action of the current of the third, fourth, or fifth order. The directions, however, of these currents, as indicated by the feeble motion of the needle, were the same as those given by the magnetizing spiral.

25. The direction of the different currents produced at the making of the battery current, as determined by these instruments, is as follows: namely, the direction of the secondary current is, as stated by Dr. Faraday, adverse to that of the primary current, and, also, the direction of each succeeding current is opposite to that of the one which produced it. We have, therefore, from these results, and those formerly obtained (III. 92.), the following series of directions of currents, one produced at the moment of beginning, and the other at that of ending of the battery current.

	At the beginning.		At the ending.	
Primary current .....	+	.....	+	
Secondary current .....	—	.....	+	
Current of the third order .	+	.....	—	
Current of the fourth order	—	.....	+	
Current of the fifth order ...	+	.....	—	

26. These two series, at first sight, may appear very different, but, with a little attention, they will be seen to be of the same nature. If we allow that the induction at the ending of a galvanic battery should be opposite to that at the beginning of the same, then the sign at the top of the second column may be called minus instead of plus, and we shall have the second series — + — + alternating precisely like the first.

27. In connexion with the results given in the last two paragraphs, it is due to Mr. Sturgeon that I should state that, in a letter addressed to me, and published in the *Annals of Electricity*, he has predicted, from his theory, that I would find on examination, the series of alternation of currents for the beginning induction which I have here given. I may, however, add, that it appears to me that this result might have been predicted without reference to any theory.† There was no reason to suppose the induction at the beginning would be different in its nature from that at the ending, and therefore the series which would be produced from the former might be immediately inferred from that belonging to the latter, by recollecting that the direction of the induction at the beginning should be opposite to that at the ending. I do not wish it to be supposed, however, from this remark, that I had, myself, drawn any inference from my experiments as to the alternations of currents which might be produced by the beginning induction; the truth is, that this action was so feeble with the arrangement of apparatus I employed, that I supposed it could not produce a series of currents of the different orders.

28. In the course of the experiments given in this section, I have found that a shock can be produced without using a coil, by arranging about ten elements of the battery in the form of a circle, and placing the helix within this. The shock was felt in the hands at the moment of closing the circuit, but the effect at opening the same was scarcely perceptible through the tongue. An attempt was also made to get indications of induction by

† We have not time at present to make any remarks on this point, but they may be expected in the next No.—EDIT.

placing the helix within a circle of dilute acid, connected with a battery instead of a coil, but the effect, if any, was very feeble.

29. I have shown, in the second number of my contributions, that if the body be introduced into a circuit with a battery of one hundred and twenty elements, without a coil, a thrilling sensation will be felt during the continuance of the current, and a shock will be experienced at the moment of interrupting the current by breaking the circuit at any point. This result is evidently due to the induction of a secondary current battery itself, and on this principle the remarkable physiological effects produced by Dr. Ure, on the body of a malefactor, may be explained. The body, in these experiments, was made to form a part of a circuit, with a compound galvanic apparatus in which a series of interruptions was rapidly made by drawing the end of the conductor over the edges of the plates of the battery. By this operation a series of induced currents must have been produced in the battery itself, the intensity of which was greater than that of the primary current.

30. In this connexion I may mention that the idea has occurred to me, that the intense shocks given by the electrical fish may possibly be from a secondary current, and that the great amount of nervous organization found in these animals may serve the purpose of a long conductor.\* It appears to me, that in the present state of knowledge, this is the only way in which we can conceive of such intense electricity being produced in organs imperfectly insulated and immersed in a conducting medium. But we have seen that an original current of feeble intensity can induce, in a long wire, a secondary current capable of giving intense shocks, although the several strands of the wire are separated from each other only by a covering of cotton thread. Whatever may be the worth of this suggestion, the secondary current affords the means of imitating the phenomena of the shock from the electrical eel, as described by Dr. Faraday. By immersing the apparatus (Fig. 1.) in a shallow vessel of water, the handles being placed at the two extremities of the diameter of the helix, and the hands plunged into the water parallel to a line joining the two poles, a shock is felt through the arms; but when the contact with the water is made in a line at right

\* Since writing the above, I found that M. Masson has suggested the same idea in an interesting thesis lately published.

angles to the last, only a slight sensation is felt in each hand, but no shock.

31. Since the publication of my last paper, I have exhibited to my class the experiment (No. III.. Sect. III.) relative to the induction at a distance on a much larger scale. All my coils were united so as to form a single length of conductor of about four hundred feet, and this was rolled into a ring of five and a half feet in diameter, and suspended vertically against the inside of the large folding door which separate the laboratory from the lecture-room. On the other side of the doors, in the lecture-room, and directly opposite the coil, was placed a helix, formed of upwards of a mile of copper wire, one-sixteenth of an inch in thickness, and wound into a hoop of four feet in diameter. With this arrangement, and a battery of one hundred and forty-seven square feet of zinc surface divided into eight elements, shocks were perceptible in the tongue, when the two conductors were separated, to the distance of nearly seven feet; at the distance of between three and four feet, the shocks were quite severe. The exhibition was rendered more interesting by causing the induction to take place through a number of persons standing in a row between the two conductors.

SECT. II.—*On apparently two kinds of Electro-dynamic Induction.*

32. The investigations arranged under this head had their origin in the following circumstances. After the publication of my last paper, I received, through the kindness of Dr. Faraday, a copy of the fourteenth series of his researches, and in this I was surprised to find a statement which appeared in direct opposition to one of the principal facts of my communication. In paragraph 59, I state, in substance, that when a plate of metal is interposed between the coil transmitting a galvanic current, and the helix placed above it to receive the induction, the shock from the secondary current is almost perfectly neutralized. Dr. Faraday, in the extension of his new and ingenious views of the agency of the intermediate particles in transmitting induction, was led to make an experiment on the same point; and apparently, under the same circumstances, he found that it "makes not the least difference, whether the intervening space between the two conductors is occupied by such insulating bodies as air,

sulphur, and shell-lac, or such conducting bodies as copper and other non-magnetic metals."

33. As the investigation of the fact mentioned above forms an important part of my paper, and is intimately connected with almost all the phenomena subsequently described in the communication, I was, of course, anxious to discover the cause of so remarkable a discrepancy. There could be no doubt of the truth of my results, since a shock from a secondary current which would paralyze the arms was so much reduced by the interposition of plates of metal, as scarcely to be felt through the tongue.

34. After some reflection, however, the thought occurred to me that induction might be produced in such a way as not to be affected by the interposition of a plate of metal. To understand this, suppose the end of a magnetic bar placed perpendicularly under the middle of a plate of copper, and a helix suddenly brought down on this; an induced current would be produced in the helix by its motion towards the plate, since the copper, in this case, could not screen the magnetic influence. Now, if we substitute for the magnet a coil through which a galvanic current is passing, the effect should be the same. The experiment was tried by attaching the ends of the helix to a galvanometer,\* and the result was as I expected; when the coil was suddenly brought down on the plate the needle swung in one direction, and when lifted up, in the other; the amount of deflection being the same, whether the plate was interposed or not.

35. It must be observed in this experiment, that the plate was at rest, and consequently did not partake of the induction produced by the motion of the helix. From my previous investigations, I was led to conclude that a different result would follow, were a current also generated in the plate by simultaneously moving it up and down with the helix. This conclusion, however, was not correct, for on making the experiment, I found that the needle was just as much affected when the plate was put in motion with the helix as when the latter alone was moved.

36. This result was so unexpected and remarkable, that it was considered necessary to repeat and vary the experi-

\* The arrangement will be readily understood by supposing in Fig. 1. the handles removed, and the ends of the helix joined to the ends of the wire of a galvanometer; also, by a plate of metal interposed between the helix and the coil.

ment in several ways. First, a coil was interposed instead of the plate, but whether the coil was at rest or in motion with the helix, with its ends separated or joined, the effect on the galvanometer was still the same; not the least screening influence could be observed. In reference to the use of the coil in this experiment, it will be recollected that I have found this article to produce a more perfect neutralization than a plate.

37. Next, the apparatus remaining the same, and the helix at rest during the experiment, currents were induced in it by moving the battery attached to the coil up and down in the acid. But in this case, as in the others, the effect on the galvanometer was the same, whether the plate or coil was interposed or not.

38. The experiment was also tried with magneto-electricity. For this purpose, about forty feet of copper wire, covered with silk were wound around a short cylinder of stiff paper, and into this was inserted a hollow cylinder of sheet copper, and into this again, a short rod of soft iron; when the latter was rendered magnetic, by suddenly bringing in contact with its two ends the different poles of two magnets, a current, of course, was generated in the wire, and this, as before, was found to affect the galvanometer to the same degree when the copper cylinder was interposed, as when nothing but the paper intervened.

39. The last experiment was also varied by wrapping two copper wires of equal length around the middle of the keeper of a horse-shoe magnet, leaving the ends of the inner one projecting, and those of the outer attached to the galvanometer. A current was generated in each by moving the keeper on the ends of the magnet, but the effect on the galvanometer was not in the least diminished by joining the ends of the inner wire.

40. At first sight, it might appear that all these results are at variance with those detailed in my last paper, relative to the effect of interposed coils and plates of metal. But it will be observed, that in all the experiments just given, the induced currents are not the same as those described in my last communication. They are all produced by motion, and have an appreciable duration, which continues as long as the motion exists. They are also of low intensity, and thus far I have not been able to get shocks by any arrangement of apparatus from currents of this kind. On the other hand, the currents produced at the moment of *suddenly* making or breaking a galvanic



current, are of considerable intensity, and exist but for an instant. From these, and other facts presently to be mentioned, I was led to suppose that there are two kinds of electro-dynamic induction; one of which can be neutralized by the interposition of a metallic plate between the conductors and the other not.

41. In reference to this surmise, it became important to examine again all the phenomena of induction at suddenly making and breaking a galvanic current.\* And in connexion with this part of the subject, I will first mention a fact which was observed in the course of the experiments given in the last section, on the direction of the induced currents of different orders. It was found, that though the indications of the galvanometer were the same as those of the spiral, in reference to the direction of the induced currents, yet they were very different in regard to the intensity of the action. Thus, when the arrangement of the apparatus was such that the induction at making the battery circuit was so feeble as not to give the least magnetism to the needle, and so powerful at the ending as to magnetize it to saturation, the indication of the galvanometer was the same in both cases.

42. Also, similar results were obtained in comparing the shock and the deflection of the galvanometer. In one experiment, for example, the shock was so feeble at making contact that it could scarcely be perceived in the fingers, but so powerful at the breaking of the circuit as to be felt in the breast; yet the galvanometer was deflected about thirty-five degrees to the right, at the beginning of the current, and only an equal number of degrees to the left, at the ending of the same.

43. In another experiment, the apparatus being the same as before, the magnetizing spiral and the galvanometer were both at once introduced into the circuit of the helix. A sewing needle being placed in the spiral, and the contact with the battery made, the needle showed no signs of magnetism, although the galvanometer was deflected thirty degrees. The needle being replaced, and the battery circuit broken, it was now found strongly magnetized, while the galvanometer was only moved about as much as before in the opposite direction.

44. Also, effects similar to those described in the last two paragraphs were produced when the apparatus was so

\* See my last page in the *Annals of "Electricity, &c.,"* vol. 4, p. 281.

arranged as to cause the induction at the beginning of the battery current to predominate. In this case the galvanometer was still nearly equally affected at making and breaking battery contact, or any difference which was observed could be referred to a variation in the power of the battery during the experiment.

45. Another fact of importance belonging to the same class has been mentioned before (24.), namely, that the actions of the currents of the third, fourth, and fifth orders produce a very small effect on the galvanometer, compared with that of the secondary current; and this is not alone on account of the diminishing power of the successive inductions, as will be evident from the following experiment. By raising the helix from the coil, in the arrangement of apparatus for the secondary current, the shock was so diminished as to be inferior to one produced by the arrangement for a tertiary current; yet, while with the secondary current the needle was deflected twenty-five degrees, with the tertiary it scarcely moved more than one degree; and with the currents of the fourth and fifth orders the deflections were still less, resembling the effect of a slight impulse given to the end of the needle.

46. With the light obtained from the foregoing experiments, I was the more fully persuaded that some new and interesting results might be obtained by a re-examination of my former experiments, on the phenomena of the interposed plate of metal, in the case where the induction was produced by making and breaking the circuit with a cup of mercury; and in this I was not disappointed. The coil (Fig. 1.) being connected with a battery of ten elements, the shocks, both at making and breaking the circuit, were very severe; and these, as usual, were almost entirely neutralised by the interposition of a zinc plate. But when the galvanometer was introduced into the circuit instead of the body, its indications were the same whether the plate was interposed or not; or, in other words, the galvanometer indicated no screening, while, under the same circumstances, the shocks were neutralized.

47. A similar effect was observed when the galvanometer and the magnetizing spiral were together introduced into the circuit. The interposition of the plate entirely neutralized the magnetizing power of the spiral, in reference to tempered steel, while the deflections of the galvanometer were unaffected.

48. In order to increase the number of facts belonging to this class, the last experiments were varied in several

ways; and first, instead of the hard steel needle, one of soft iron wire was placed in the spiral, with a small quantity of iron filings almost in contact with one of its ends. The plate being interposed, the small particles of iron were attracted by the end of the needle, indicating a feeble, temporary developement of magnetism. Hence the current which moves the needle, and is not neutralized by the interposed plate, also feebly magnetizes soft iron, but not hard steel.

49. Again, the arrangement of apparatus being as in paragraph 46, instead of a plate of zinc, one of cast iron, of about the same superficial dimensions, but nearly half an inch thick, was interposed; with this the magnetizing power of the spiral, in reference to tempered steel, was neutralized; and, also, the action of the galvanometer was much diminished.

50. Another result was obtained by placing in the circuit of the helix (fig. 1.), at the same time, the galvanometer, the spiral, and a drop of distilled water; with these the magnetizing power of the spiral was the same as without the water, but the deflection of the galvanometer was reduced from ten to about four degrees. In addition to these, the body was also introduced into the same circuit; the shocks were found very severe, the spiral magnetized needles strongly, but the galvanometer was still less moved than before. The current of low intensity, which deflects the needle of the galvanometer in these instances, was partially intercepted by the imperfect conduction of the water and the body.

51. To exhibit the results of these experiments with still more precision, an arrangement of apparatus was adopted similar to that used by Dr. Faraday, and described in the fourteenth series of his researches, namely, a double galvanometer was formed of two separate wires of equal length and thickness, wound together on the same frame; and, also, a double magnetizing spiral was prepared by winding two equal wires around the same piece of hollow straw. Coil No. 1\*, connected with the battery, was supported perpendicularly on a table, and coils Nos. 3 and 4 were placed parallel to this, one on each side, to receive the induction, the ends of these being so joined with those of the galvanometer and the spiral that the induced current from the one coil would pass through the

\* See *Annals*, 8g. 2, plate viii., vol. 17,

two instruments, in an opposite direction to that of the current from the other coil. The two outside coils were then so adjusted, by moving them to and from the middle coil, that the induced currents perfectly neutralized each other in the two instruments, and the needle of the galvanometer and that in the spiral were both unaffected when the circuit of the battery was made and broken. With this delicate arrangement the slightest difference in the action of the two currents would be rendered perceptible; but when a zinc plate was introduced, so as to screen one of the coils, the needle of the galvanometer still remained perfectly stationary, indicating not the least action of the plate, while the needle in the spiral became powerfully magnetic. When, however, a plate of iron was interposed instead of the one of zinc, the needle of the galvanometer was also affected.

52. From the foregoing results it would seem that the secondary current, produced at the moment of suddenly beginning or ending of a galvanic current, by making and breaking contact with a cup of mercury, consists of two parts, which possess different properties. One of these is of low intensity, can be interrupted by a drop of water, does not magnetize hardened steel needles, and is not screened by the interposition of a plate of any metal, except iron, between the conductors. The other part is of considerable intensity, is not intercepted by a drop of water, develops the magnetism of hardened steel, gives shocks, and is screened or neutralized by a closed coil, or a plate of any kind of metal. Also, the induced current produced by moving a conductor towards or from a battery current, and that produced by the movement up and down of a battery in the acid, are of the nature of the first-mentioned part, while the currents of the third, fourth, and fifth orders partake almost exclusively of the properties of the second part.

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53. The principal facts and conclusions of this section were announced to the Society in October 1839, and again presented in the form in which they were detailed in the *Annals of Electricity, Magnetism and Chemistry*, &c., vol. IV., p. 281. Since then, however, I have had leisure to examine the subject more attentively, and after a careful comparison of these results with those before given, I have obtained the more definite views of the phenomena which are given in the next section.

**SECT. III.—Theoretical Considerations relating to the Phenomena described in this and the preceding Communications. Read November 20, 1840.**

54. The experiments given in the last No. of my contributions were merely arranged under different heads, and only such inferences drawn from them as could be immediately deduced without reference to a general explanation. The addition, however, which I have since made to the number of facts, affords the means of a wider generalization; and after an attentive consideration of all the results given in this and the preceding papers, I have come to the conclusion that they can all be referred to the simple laws of the induction at the beginning and the ending of a galvanic current.

55. In the course of these investigations the limited hypothesis which I have adopted have been continually modified by the development of new facts, and therefore my present views, with the further extension of the subject, may also require important corrections. But I am induced to believe, from its exact accordance with all the facts, so far as they have been compared, that if the explanation I now venture to give be not absolutely true, it is so, at least, in approximation, and will therefore be of some importance in the way of suggesting new forms of experiment, or as a first step towards a more perfect generalization.

56. To render the laws of induction at the beginning and the ending of a galvanic current more readily applicable to the explanation of the phenomena, they may be stated as follows:—1. During the time a galvanic current is increasing in quantity in a conductor, it induces, or tends to induce, a current in an adjoining parallel conductor in an opposite direction to itself. 2. During the continuance of the primary current in full quantity, no inductive action is exerted. 3. But when the same current begins to decline in quantity, and during the whole time of its diminishing, an induced current is produced in an opposite direction to the induced current at the beginning of the primary current.

57. In addition to these laws, I must frequently refer to the fact, that *when the same quantity of electricity in a current of short duration is passed through a galvanometer, the deflecting force on the needle is the same, whatever be the intensity of the electricity.* By intensity is here un-

derstood the ratio of a given quantity of force to the time in which it is expended<sup>\*</sup>; and according to this view, the proposition stated is an evident inference from dynamic principles. But it does not rest alone on considerations of this kind, since it has been proved experimentally by Dr. Faraday, in the third series of his researches.<sup>†</sup>

58. In order to form a definite conception of the several conditions of the complex phenomena which we are about to investigate, I have adopted the method often employed in physical inquiries, of representing the varying elements of action by the different parts of a curve. This artifice has been of much assistance to me in studying the subject, and without the use of it at present, I could scarcely hope to present my views in an intelligible manner to the Society.

59. After making these preliminary statements, we will now proceed to consider the several phenomena; and, first, let us take the case in which the induction is most obviously produced in accordance with the laws as above stated (56.), namely, by immersing a battery into the acid, and also by withdrawing it from the same. During the time of the descent of the battery into the liquid, the conductor connected with it is constantly receiving additional quantities of current electricity, and each of these additions produces an inductive action on the adjoining secondary conductor. The amount, therefore, of induced current produced during any moment of time will be just in proportion to the corresponding increase in the current of the battery during the same moment. Also, the amount of induction during any moment while the current of the battery is diminishing in quantity will be in proportion to the decrease during the same moment.

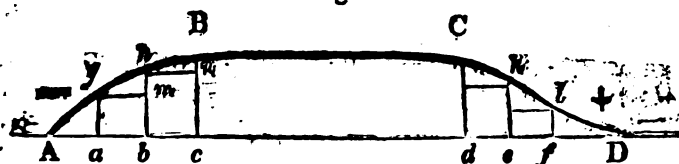
60. The several conditions of this experiment may be represented by the different parts of the curve, A, B, C, D, fig. 2, in which the distances, A a, A b, A c, represent the times during which the battery is descending to different depths into the acid; and the corresponding ordinates, a g, b h, c B, represent the amount of current electricity in the battery conductor corresponding to these times. The differences of the ordinates, namely, a g, m h, n B, express the increase in the quantity of the battery current during the corresponding moments of time repre-

<sup>\*</sup> Or more correctly speaking, the ratio of two quantities of the same species representing the force and time.

<sup>†</sup> See *Annals of Electricity*, &c. vol. 1., p. 52, for our opinion of this inference.—EDIT.

mented by  $Aa$ ,  $ab$ ,  $bc$ ; and since the inductive actions (59.) are just in proportion to these increases, the same differences will also represent the amount of induced action exerted on the secondary conductor during the same moment of time.

Fig. 2.



61. When the battery is fully immersed in the acid, or when the current in the conductor has reached its state of maximum quantity, and during the time of its remaining constant, no induction is exerted; and this condition is expressed by the constant ordinates of the part of the curve  $BC$ , parallel to the axis. Also, the inductive action produced by each diminution of the battery current, while the apparatus is in the progress of being drawn from the acid, will be represented by the differences of the ordinates at the other end,  $CD$ , of the curve.

62. The sum of the several increments of the battery current, up to its full development, will be expressed by the ordinate  $cB$ , and this will, therefore, also represent the whole amount of inductive action exerted in one direction at the beginning of the primary current; and, for the same reason, the equal ordinate,  $Cd$ , will represent the whole induction in the other direction at the ending of the same current. Also, the whole time of continuance of the inductive action at the beginning and ending will be represented by  $Ae$  and  $dD$ .

63. If we suppose the battery to be plunged into the acid to the same depth, but more rapidly than before, then the time represented by  $Ae$  will be diminished, while the whole amount of inductive force expended remains the same; hence, since the same quantity of force is exerted in a less time, a greater intensity of action will be produced (57.), and consequently a current of more intensity, but of less duration, will be generated in the secondary conductor. The intensity of the induced currents will, therefore, evidently be expressed by the ratio of the ordinate  $cB$  to the abscissa  $Ae$ . Or, in more general and definite terms, the intensity of the inductive action at any

moment of time will be represented by the ratio of the rate of increase of the ordinate to that of the abscissa for that moment\*.

64. It is evident from the last paragraph, that the greater or less intensity of the inductive action will be immediately presented to the eye, by the greater or less obliquity of the several parts of the curve to the axis. Thus, if the battery be suddenly plunged into the acid for a short distance, and then gradually immersed through the remainder of the depth, the varying action will be exhibited at once by the form of A B, the first part of the curve, fig. 2. The steepness of the part A g will indicate an intense action for a short time A a, while the part g B denotes a more feeble induction during the time represented by a c. In the same way, by drawing up the battery suddenly at first, and afterwards slowly, we may produce an inductive action such as would be represented by the parts between C and D of the ending of the curve.

65. Having thus obtained representations of the different elements of action, we are now prepared to apply these to the phenomena. And, first, however varied may be the intensity of the induction expressed by the different parts of the two ends of the curve, we may immediately infer that a galvanometer, placed in the circuit of the secondary conductor, will be equally affected at the beginning and ending of the primary current; for, since the deflection of this instrument is due to the whole amount of a current, whatever may be its intensity (57.), and since the ordinates c B and C d are equal, which represent the quantity of induction in the two directions, and, consequently, the amount of the secondary current, therefore the deflection at the beginning and ending of the battery current will, in all cases, be equal. This inference is in strict accordance with the results of experiment; for however rapidly or slowly we may plunge the battery into the acid, and however irregular may be the rate at which it is drawn out, still if the whole effect be produced within the time of one swing of the needle, the galvanometer is deflected to an equal degree.

\* According to the differential notation, the intensity will be expressed by  $\frac{dy}{dx}$ . In some cases the effect may be proportional to the intensity multiplied by the quantity, and this will be expressed by  $\frac{dy^2}{dx}$ , x and y representing, as usual, the variable abscissa and ordinary.



66. Again, the intensity of one part of the inductive action, for example that represented by  $A g$ , may be supposed so great as to produce a secondary current capable of penetrating the body, and of thus producing a shock\* while the other parts of the action, represented by  $g B$  and  $C D$ , are so feeble as to affect the galvanometer only. We would then have a result the same as one of those given in the last section (42.), and which was supposed to be produced by two kinds of induction; for if the shock were referred to as the test of the existence of an induced current, one would be found at the beginning only of the battery current, while, if the galvanometer were consulted, we would perceive the effects of a current as powerful at the ending as at the beginning.

67. The results mentioned in the last paragraph cannot be obtained by plunging a battery into the acid; the formation of the current in this way is not sufficiently rapid to produce a shock. The example was given to illustrate the manner in which the same effect is supposed to be produced, in the case of the more sudden formation of a current, by plunging one end of the conductor into a cup of mercury permanently attached to a battery already in the acid, and in full operation. The current, in this case, rapid as may be its development, cannot be supposed to assume *per saltum* its maximum state of quantity; on the contrary, from the general law of continuity, we would infer that it passes through all the intermediate states of quantity, from that of no current, if the expression may be allowed, to one of full development; there are, however, considerations of an experimental nature which would lead us to the same conclusion (18,90.), and also to the further inference that the *decline* of the current is not instantaneous. According to this view, therefore, the inductive actions at the beginning and ending of a primary current, of which the formation and interruption is effected by means of the contact with a cup of mercury, may also be represented by the several parts of the curve, fig. 2.

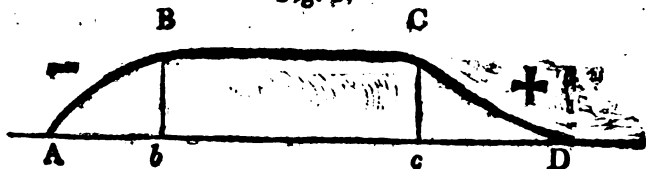
68. We have now to consider how the rate of increase or diminution of the current, in the case in question, can be altered by a change in the different parts of the apparatus; and, first, let us take the example of a single battery and a short conductor, making only one or two

\* The shock depends more on the intensity than on the quantity. See paragraph 13.

turns around the helix ; with this arrangement a feeble shock, as we have seen (11.), will be felt at the making, and also at the breaking of the circuit. In this case it would seem that almost the only impediment to the most rapid development of the current would be the resistance to conduction of the metal ; and this we might suppose would be more rapidly overcome by increasing the tension of the electricity ; and, accordingly, we find, that if the number of the elements of the battery be increased, the shock at making the circuit will also be increased, while at breaking the circuit will remain nearly the same. To explain, however, this effect more minutely, we must call to mind the fact before referred to (17), that when the poles of a compound battery are not connected, the apparatus acquires an accumulation of electricity, which is discharged at the first moment of contact, and which in this case would more rapidly develop the full current, and hence produce the more intense action on the helix at making the circuit.

69. The shock, and also the deflection of the needle, at breaking the circuit with a compound battery and a short coil (9), appears nearly the same as with a battery of a single element, because the accumulation just mentioned, in the compound battery, is discharged almost instantly, and, according to the theory (71.) of the galvanic current, leaves the constant current in the conductor nearly in the same state of quantity as that which would be produced by a battery of a single element ; and hence the conditions of the ending of the current are the same in both cases. Indeed, in reference to the ending induction, it may be assumed as a fact which is in accordance with all the experiments (9, 13, 73, 74, 75, 76, &c.), as well as with theoretical considerations\*, that when the circuit is broken by a cup of mercury, the rate of the diminution of the current, within certain limits, remains the same, however the intensity of the electricity or the length of the conductor may be varied.

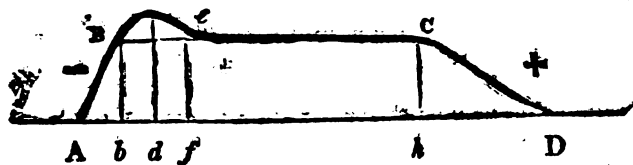
Fig. 3.



\* See the theory of Ohm, — Taylor's Scientific Memoirs, vol. 1, p. 312, 311, and vol. II. p. 61.

70. The several conditions of the foregoing examples are exhibited by the parts of the curves, figs. 3 and 4. The gradual development of the current in the short conductor, with a single battery, and the gradual decline of the same, are represented by the gentle rise of A B and fall of C D, fig. 3; while, in the next fig. (4.), the sudden rise of A B indicates the intensity which produces the increased shock, after the number of elements of the battery has been increased. The accumulation of the electricity, which almost instantly subsides, is represented by the part B c e, fig. 4; and from this we see, at once, that although the shock is increased by using the compound battery, yet the needle of the galvanometer will be deflected only to the same number of degrees, since the parts B c and c e give inductive actions in contrary directions, and both within the time of a single swing of the needle, and, consequently, will neutralize each other. The resulting deflecting force will, therefore, be represented by e f, which is equal to C A, or to b B, in fig. 3.

Fig. 19.



The intensity of the shock at the breaking is represented as being the same in the two figures, by the similarity of the rate of descent of the part C D of the curve in each.

71. We have said (69.) that the quantity of current electricity in a short conductor and a compound battery, after the first discharge, is nearly the same as with a single battery. The exact quantity, according to the theory of Ohm, in a unit of length of the conductor, is given by the formula

$$\frac{n A}{r n + R}.$$

In this  $n$  represents the number of elements;  $A$ , the electromotive force of one element;  $r$ , the resistance to conduction of one element; and  $R$ , the length of the conductor, or rather its resistance to conduction in terms of  $r$ . Now, when  $R$  is very small, in reference to  $r n$ , as

is the case with a very short metallic conductor, it may be neglected, and then the expression becomes

$$\frac{n A}{r n} \text{ or } \frac{A}{r};$$

and since this expresses the quantity of current electricity in a unit of the length of the circuit, with either a single or a compound battery, therefore, with a short conductor, the quantity of current electricity in the two cases is nearly the same.

72. Let us next return to the experiment with a battery of a single element (68.), and instead of increasing the intensity of the apparatus, as in the last example, let the length of the conductor be increased; then the intensity of the shock at the beginning of the current, as we have seen (14.), will be diminished, while that of the one at the ending will be increased. That the shock should be lessened at the beginning, by increasing the length of the conductor, is not surprising, since, as we might suppose, the increased resistance to conduction would diminish the rapidity of the development of the current. But the secondary current, which is produced in the conductor of the primary current itself, as we have seen (19.), is the principal cause which lessens the intensity of the shock, and the effect of this, as will be shewn hereafter, may also be inferred from the principles we have adopted.

73. The explanation of the increased shock at the moment of breaking the circuit with the long conductor, rests on the assumption before-mentioned (69.), that the velocity of the diminution of a current is nearly the same in the case of a long conductor as in that of a short one. But, to understand the application of this principle more minutely, we must refer to the change which takes place in the quantity of the current in the conductor by varying its length; and this will be given by another application of the formula before stated (71). This, in the case of a single battery, in which  $n$  equals unity, becomes

$$\frac{A}{r + R};$$

and since this, as will be recollected, represents the quantity of current electricity in a unit of length; of the conductor, we readily infer from it that, by increasing the length of the conductor, or the value of  $R$ , the quantity

of current in a unit of the length is lessened. And if the resistance of a unit of the length of the conductor were very great in comparison with that of  $r$  (the resistance of one element of the battery), then the formula would become

$$\frac{A}{R}$$

or the quantity in a single unit of the conductor would be inversely as its entire length, and hence the amount of current electricity in the whole conductor would be a constant quantity, whatever might be its length. This, however, can never be the case in any of our experiments, since in no instance is the resistance of  $R$  very great in reference to  $r$ , and therefore, according to the formula (73.), the whole quantity of current electricity in a long conductor is always somewhat greater than in a short one.

74. Let us, however, in order to simplify the conditions of the induction at the ending of a current, suppose that the quantity in a unit of the conductor is inversely as its whole length, or in other words, that the quantity of current electricity is the same in a long conductor as in a short one; and let us also suppose, for an example, that the length of the spiral conductor, fig. 1, was increased from one spire to twenty spires; then, if the velocity of the diminution of the section of the current is the same (69.) in the long conductor as in the short one; the shock which would be received by submitting the helix to the action of one spire of the long coil would be nearly of the same intensity as that from one spire of the short conductor; the quantity of induction, however, as shown by the galvanometer, should be nearly twenty times less; and these inferences I have found in accordance with the results of experiments (75). If, however, instead of placing the helix on one spire of the long conductor, it be submitted at once to the influence of all the twenty spires, then the intensity of the shock should be twenty times greater, since twenty times the quantity of current electricity collapses, if we may be allowed the expression, in the same time, and exerts at once all its influence on the helix. If, in addition to this, we add the consideration, that the whole quantity of current electricity in a long conductor is greater than that in a short one (73.), we shall have a further reason for the increase of the terminal shock, when we increase the length of the battery conductor.

75. The inference given in the last paragraph, relative to the change in the quantity of the induction, but not in the intensity of the shock from a single spire, by increasing the whole length of the conductor, is shown to be true by repeating the experiment described in paragraph 13. In this, as we have seen, the intensity of the shock remained the same, although the length of the circuit was increased by the addition of coil No. 2. When, however, the galvanometer was employed in the same arrangement, the whole quantity of induction, as indicated by the deflection of the needle, was diminished almost in proportion to the increased length of the circuit. I was led to make this addition to the experiment (13.) by my present views.

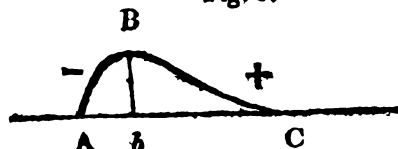
76. The explanation given in paragraph 74 also includes that of the peculiar action of a long conductor, either coiled or extended, in giving shocks and sparks from a battery of a single element, discovered by myself in 1831 (see *Contrib*, No. II.). The induction, in this case, takes place in the conductor of the primary current itself, and the secondary current which is produced is generated by the joint action of each unit of the length of the primary current. Let us suppose, for illustration, that the conductor was at first one foot long, and afterwards increased to twenty feet. In the first case, because the short conductor would transmit a greater quantity of electricity, the secondary current produced by it would be one of considerable quantity or power to deflect a galvanometer; but it would be of feeble intensity, for although the primary current would collapse with its usual velocity (69.), yet, acting on only a foot of conducting matter, the effect (74) would be feeble. In the second case, each foot of the twenty feet of the primary current would severally produce an inductive action of the same intensity as that of the short conductor, the velocity of collapse being the same; and as they are all at once exerted on the same conductor, a secondary current would result, of twenty times the intensity of the current in the former case.

77. To render this explanation more explicit, it may be proper to mention, that a current produced by an induction on one part of a long conductor of uniform diameter, must exist, of the same intensity, in every other part of the conductor; hence the action of the several units of length of the primary current must enforce each other, and produce the same effect on its own conductor that the same current would if it were in a coil, and acting on a helix.

I need scarcely add, that in this case, as in the given paragraph 74, the whole amount of induction is greater with the long conductor than with the short one, because the quantity of current electricity is greater in the former than in the latter.

78. We may next consider the character of the secondary current, in reference to its action in producing a tertiary current in a third conductor. The secondary current consists, as we may suppose, in the disturbance, for an instant, of the natural electricity of the metal, which, subsiding, leaves the conductor again in its natural state; and whether it is produced by the beginning or ending of a primary current, its nature, as we have seen (22), is the same. Although the time of continuance of the secondary current is very short, still we must suppose it to have some duration, and that it increases, by degrees, to a state of maximum development, and then diminishes to the normal condition of the metal of the conductor; the velocity of its development, like that of the primary current, will depend on the intensity of the action by which it is generated, and also, perhaps, in some degree, on the resistance of the conductor; while, agreeably to hypothesis we have assumed (69), the velocity of its diminution is nearly a constant quantity, and is not affected by changes in these conditions; hence, if we suppose the induction which produces the secondary current to be sufficiently intense, the velocity of its development will exceed that of its diminution, as in the example of the primary current from the intense source of the compound battery of many elements. Now this is the case with the inductions which produce currents of the different orders, capable of giving shocks or of magnetizing steel needles: the secondary currents from these are always of considerable intensity, and hence their rate of development must be greater than that of their diminution; and, consequently, they may be represented by a curve of the form exhibited in fig. 5, in which there is no constant part, and in which the steepness

Fig. 5.



of the curve fig. 20, rendering it still more gradual in its

of A B is greater than that of B C. There are, however, other considerations, which will be noticed hereafter (89.), which may affect the

descent, or, in other words, which tend to diminish the intensity of the ending induction of the secondary current.

79. It will be seen at once, by an inspection of the curve, that the effect produced, in a third conductor, and which we have called a tertiary current, is not of the same nature as that of a secondary current. Instead of being a single development in one direction, it consists of two instantaneous currents, one produced by the induction of A B, and the other, by that of B C, in opposite directions, of equal quantities, but of different intensities. The whole quantity of induction in the two directions will each be represented by the ordinate B *b*, and hence they will nearly neutralize each other, in reference to their action on the galvanometer, in the circuit of the third conductor. I say they will *nearly* neutralize each other, because, although they are equal in quantity, they do not both act in absolutely the same moment of time. The needle will, therefore, be slightly affected: it will be impelled in one direction, say to the right, by the induction of A B, but before it can get fairly under way, it will be arrested, and turned in the other direction, by the action of B C. This inference is in strict accordance with observation: the needle, as we have seen (24.), starts from a state of rest, with a velocity which, apparently, would send it through a large arc; but before it has reached, perhaps, more than half a degree, it suddenly stops, and turns in the other direction. As the needle is first affected by the action of A B, it indicates a current in the adverse direction to the secondary current.

80. Although the two inductions in the tertiary conductor nearly neutralize each other, in reference in the indications of the galvanometer, yet this is far from being the case with regard to the shocks, and the magnetization of steel needles. These effects may be considered as the results alone of the action of A B; the induction of B C being too feeble in intensity to produce a tertiary current of sufficient power to penetrate the body, or overcome the coercive power of the hardened steel. Hence, in reference to the shock and magnetization of the steel needle, we may entirely neglect the action of B C, and consider the tertiary excitement as a single current, produced by the action of A B; and, because this is the beginning induction (56.), the tertiary current must be in an opposite direction to the secondary. For a similar reason, a current of the third order should produce in effect a single current of the



fourth order, in a direction opposite to that of the current which produced it, and so on : we have here, therefore, a simple explanation of the extraordinary phenomenon of the alternation of the directions of the currents, of the different orders, as given in this and the preceding paper. See paragraph 25.

81. The operation of the interposed plate (32, 47, 48, &c.), in neutralizing the shock, and not affecting the galvanometer, can also be readily referred to the same principles. It is certain that an induced current is produced in the plate (III. 65.), and that this must react on the secondary in the helix ; but it should not alter the total amount of this current, since, for example, at the ending induction, the same quantity of current is added to the helix, while the current in the plate is decreasing, as is subtracted while the same current is increasing. To make this more clear, let the inductive actions of the interposed current be represented by the parts of the curve, fig. 5. The induction represented by A B will re-act on the current in the helix, and diminish its quantity, by an amount represented by the ordinate  $b B$  ; but the induction represented by B C will act, in the next moment, on the same current, and increase its quantity by an equal amount, as represented by the same ordinate B  $b$  ; and since both actions take place within a small part of the time of a single swing of the needle, the whole deflection will not be altered, and consequently, as far as the galvanometer is concerned, the interposition of the plate will have no perceptible effect.

82. But the action of the plate on the shock, and on the magnetization of tempered steel, should be very different ; for although the quantity of induction in the helix may not be changed, yet its intensity may be so reduced, by the adverse action of the interposed current, as to fall below that degree which enables it to penetrate the body, or overcome the coercive force of the steel. To understand how this may be, let us again refer, for example, to the induction which takes place at the ending of a battery current : this will produce, in both the helix and the plate, a momentary current, in the direction of the primary current, which we have called *plus* ; the current in the plate will react on the helix, and tend to produce it in two inductions, which, as before, may be represented by A B and B C of the curve, fig. 5 ; the first of these, A B, will be an intense action (78.), in the *minus* direction, and will there-

fore tend to neutralize the intense action of the primary current on the helix; the second (B C) will add to the helix an equal quantity of induced current, but of a much more feeble intensity, and hence the resulting current in the helix will not be able to penetrate the body; no shock will be perceived, or at least a very slight one, and the phenomena of screening will be exhibited.

83. When the plate of metal is placed between the conductors of the second and third orders, or between those of the third and fourth, the action is somewhat different, although the general principle is the same. Let us suppose the plate interposed between the second and third conductors; then the helix, or third conductor, will be acted on by four inductions, two from the secondary current and two from the current in the plate. The direction and character of these will be as follows, on the supposition that the direction of the secondary current is itself *plus*:

The ending interposed . . . feeble and . . . *minus*.

The beginning secondary . . intense and . . *minus*.

The ending secondary . . . feeble and . . . *plus*.

The beginning interposed . . intense and . . *plus*.

Now if the action, on the third conductor, of the first and third of the above inductions be equal in intensity and quantity, they will neutralize each other; and the same will also take place with the action of the second and fourth if they be equal, and hence, in this case, neither shock nor motion of the needle of the galvanometer would be produced. If these inductions are not precisely equal, then only a partial neutralization will take place, and the shock will only be diminished in power; and, also, perhaps, the needle will be very slightly affected.

84. If, in the foregoing exposition, we throw out of consideration the actions of the feeble currents which cannot pass the body, and, consequently, are not concerned in producing the shock, then the same explanation will still apply which was given in the last paper (III. 94.); namely, in the above example, the helix is acted on by the minus influence of the secondary, and the plus influence of the interposed current.

85. We are now prepared to consider the effect on the helix (fig. 1.) of the induced currents produced in the conductor of the primary current itself. These are true secondary currents, and are almost precisely the same in

their action as those in the interposed plate. Let us first examine the induced current at the beginning of the primary, in the case of a long coil and battery of a single element; its action on the helix may be represented by the parts of the curve, fig. 5. The first part, A B, will produce an intense induction opposite to that of the primary current; and hence the action of the two will tend to neutralize each other, and no shock, or a very feeble one, will be produced. The ending action of the same induced current, which is represented by B D, restores to the helix the same quantity of current electricity (but in a feeble state) which was neutralized by A B, and hence the needle of the galvanometer will be as much affected as if this current did not exist. These inferences perfectly agree with the experiment given in paragraph 19. In this, when the ends of the interposed coil were joined so as to neutralize the induced current in the long conductor, the shock at the beginning of the primary current was nearly as powerful as with a short conductor, while the amount of deflection of the galvanometer was unaffected by joining the ends of the same coil.

86. At first sight it might appear that any change in the apparatus which might tend to increase the induction of the primary current (16) would also tend to increase, in the same degree, the adverse secondary in the same conductor; and that hence the neutralization mentioned in the last paragraph would take place in all cases; but we must recollect, that if a more full current be suddenly formed in a conductor of a given thickness, the adverse current will not have, as it were, as much space for its development, and, therefore, will have less power in neutralizing the induction of the primary than before. But there is another, and, perhaps a better reason, in the consideration, that in the case of the increase of the number of elements of the battery, although the rapidity of the development of the primary current is greater, yet the increased resistance which the secondary meets with, in its motion against the action of the several elements, will tend to diminish its effect. Also, by diminishing the length of the primary current, we must diminish (76.) the intensity of the secondary, so that it will meet with more resistance in passing the acid of the single battery, and thus its effects be diminished.

87. The action of the secondary current, in the long coil at the ending of the primary current, should a.s.o, at

first sight, produce the same screening influence as the current in the interposed plate ; but, on reflection, it will be perceived that its action in this respect must be much more feeble than that of the similar current at the beginning ; the latter is produced at the moment of making contact, and hence it is propagated in a continuous circuit of conducting matter, while the other takes place at the *rupture* of the circuit, and must therefore be rendered comparatively feeble by being obliged to pass through a small portion of heated air ; very little effect is therefore produced on the helix by this induction (19). The fact that this current is capable of giving intense shocks, when the ends of a long wire, which is transmitting a primary current, are grasped at the time of breaking the circuit, is readily explained, since, in this case, the body forms, with the conductor, a closed circuit, which permits the comparatively free circulation of the induced current.

88. It will be seen that I have given a peculiar form to the beginning and ending of the curves, figs. 2, 3, &c. These are intended to represent the variations which may be supposed to take place in the rate of increase and decrease of the quantity of the current, even in the case where the contact is made and broken with mercury. We may suppose, from the existence of analogous phænomena in magnetism, heat, &c., that the development of the current would be more rapid at first than when it approximates what may be called the state of current saturation, or when the current has reached more nearly the limit of capacity of conduction of the metal. Also, the decline of the current may be supposed to be more rapid at the first moment than after it has lost somewhat of its intensity, or sunk more nearly to its normal state. These variations are indicated by the rapid rise of the curve, fig. 2, from A to *g*, and the more gradual increase of the ordinates from *h* to B ; and by the rapid diminution of the ordinates between C and *l*, and the gradual decrease of those towards the end of the curve.

89. These more minute considerations, relative to the form of the curve, will enable us to conceive how the time of the ending of the secondary current, as we have suggested (78.) may be prolonged beyond that of the natural subsidence of the disturbance of the electricity of the conductor on which this current depends. If the development of the primary current is produced by equal increments in equal times, as would be the case in plunging

the battery (59.) into the acid with a uniform velocity, then the part *A B* of the curve fig. 2 would be a straight line, and the resulting secondary current, after the first instant, would be one of constant quantity during nearly the whole time represented by *A c*; but if the rate of the development of the primary current be supposed to vary in accordance with the views we have given in the last paragraph, then the quantity of the secondary current will begin to decline before the termination of the induction, or as soon as the increments of the primary begin to diminish; and hence the whole time of the subsidence of the secondary will be prolonged, or the length of *b C*. fig. 5 will be increased, the descent of *B C* be more gradual, and the intensity of the ending induction of the secondary current be diminished (see last part of paragraph 78.)

90. Besides the considerations we have mentioned (88.), there are others of a more obvious character, which would also appear to affect the form of particular parts of the curve. And first we might perhaps make a slight correction in the drawing of fig. 2, 3, &c., at the point *A*, in consideration of the fact that the very first contact of the end of the conductor with the surface of the mercury is formed by a point of the metal, and hence the increment of development should be a little less rapid at the first moment than after the contact has become larger; or in other words, the curve should perhaps start a little less abruptly from the axis at the point *A*. Also Dr. Page has stated\* that he finds the shock increased by spreading a stratum of oil over the surface of the mercury; in this case it is probable that the termination of the current is more sudden, on account of the prevention of the combustion of the metal by means of the oil, and the fact that the end of the conductor is drawn up into a non-conducting medium.

91. The time of the subsidence of the current, when the circuit is broken by means of a surface of mercury, is very small, and probably does not exceed the ten thousandth part of a second; but even this is an appreciable duration, since I find that the spark at the ending presents the appearance of a band of light of considerable length, when viewed in a mirror revolving at the rate of six hundred times in a second; and I think the variations in the time of ending of the current under different conditions may be detected by means of this instrument.

\* *Silliman's Journal*.

92. Before concluding this communication, I should state that I have made a number of attempts to verify the suggestion given in my last paper (III 127.), that an inverse induction is produced by a galvanic current by a change in the distance of the conductors, but without success. These attempts were made before I had adopted the views given in this section, and since then I have found (80.) a more simple explanation of the alteration of the currents.

93. In this Number of my Contributions, the phænomena exhibited by the galvanic apparatus have alone been discussed. I have, however, made a series of experiments on the induction from ordinary electricity, and the reaction of soft iron on currents, and I think that the results of these can also be referred to the simple principles adopted in this paper; but they require further examination before submitting to the public.

*A Comparative Examination of the Sugar Cane grown at the Antilles and in France, followed by Considerations on the Manufacture of Sugar.—Second Memoir.*  
By M. OSMIN HERVY, Professor of Chemistry in the School of Pharmacy, Paris. \*

This work had been remitted to us by M. Hervy some days previous to the frightful event which removed him from the ranks of science. We hasten to publish it, happy in having it still in our power to render this late homage to the memory of this excellent and unfortunate young man.

The richness of the sugar cane (*arundo saccharifera*) being at the present day well known, a colonial manufacture better understood ought to cause by an augmentation of its products in sugar, a great commercial movement. It is that we may hasten as much as lies in our power the realization of this laudable hope conceived by the planters, that we now add a few words to a work on the sugar cane published by us in the month of September last. The sugar canes of which we have made use in these new experiments are no longer those grown on the

wet and burning soil of the Antilles but those of France, the one under the protection of the greenhouse, the other in the open land.

Salts, and other organic matters foreign to sugar, exist in the French canes, in far more considerable proportions than in the canes of the colonies; but it is then a fact of little importance, and only worthy of being disposed of in the same manner as their richness in sugar; it matters little in effect, to the creole planter, whether cane grown in a greenhouse be more or less rich in sugar than his own. That which to him is of the greatest importance is to know first,—That the molasses does not pre-exist in the cane during its vegetation; that what we have found as a constituent part of the dry canes which we had received from the colonies, prove that they had undergone some alteration which must be due either to dessication, or to some effect produced by the voyage. Secondly, that molasses may be formed by the reaction of certain bodies on the crystallizable sugar.

The sugar canes which have been the objects of our researches had been cultivated either in the greenhouses of the royal domain of Neuilly,\* or in the open air in the botanic garden of Bordeaux. M. Gachet, professor of botany, cheerfully offered to send them to us on our last visit to that place.

The sugar canes grown in open land were of small diameter, but of a yellow gold colour, stripped of their leaves to the summit, an evident proof of a perfect maturity: their savour was sweet, aromatic, and very agreeable.

The canes grown in the greenhouse, have a much greater diameter. Their knots were at very diversified distances, their colour green or opal yellow, some were stripped of their leaves, others still reserved them, and let us here remark that the age of these canes varied from a few months to three years. Each year of vegetation was marked by a protuberance, marking the point where vegetation had been renewed the year following. Their savour which is sweet, aromatic, and very agreeable, has been recognized by the creoles, altogether sugary and little different in flavour from canes grown in the Antilles. The juice of our canes was remarkable for its different

\* We are under considerable obligations to M. Jaques, gardener in chief to the Royal Society of Agriculture on the Seine.

degrees of density, from 103 to 108 (5 to 12 Baumi), according as it was obtained from the foot of the cane or the upper part of it.

Sugar exists in its crystallizable state in the higher knots of the cane, that is to say, at the earliest period of vegetation, precisely as it does in the lower knots which are of several years growth. It appears very evident, in our opinion, that the crystallizable sugar is not a secondary product, but, in reality, a primitive product, from the secret elaboration, to the state of crystallizable sugar, to the moment of the formation of the vessels in which it is enclosed. It appears even certain—we shall attempt likewise to prove it—that it exists in the state of crystallizable sugar from the moment of the blossoming and fructification of the cane.\*

The ideas which we have now detailed, on the absence of liquid sugar in the sugar cane, have for their basis the following experiments :

Pieces of sugar cane, cut into very thin slices, were dried, some in vacuo, and others in a dry air, at a temperature of 61° centigrades—in fifteen minutes a complete dessication took place without altering the sugar. These dried slices were then pulverized; the cane now remains, very little hygrometric. From the powder of cane, de-

\* These facts seem to us to establish a very decided limit between the formation in vegetables of the sugar of cane (crystallizable) and the formation of the sugar of raisins; in fact, we never find sugar in fruits at their birth, and we see it produced during the act of maturation; there is then a very decided difference between the natural formation of the sugar of canes, and that of sugar of raisins; those parts of plants which contain the first have, from their infancy, as the cane and the beet, a sweet flavour evidently saccharine. The parts of plants which contain the second have in their birth or infancy, a harsh bitter flavour, with no traces of sugar.

The microscopic examination of the sugar cane, has shewn us that this beautiful cellular arrangement, undergoes no change in its organization during any period of its vegetation; the nascent cane like that more advanced in age, presents the same tubes, the same groups of ligneous fibre; and we may truly say, that the anatomical organization of the cane is as beautiful and as simple as it is possible to conceive of. M. Fremy has observed on the contrary, that the disposition of the cellules change from day to day in the aridifrons fruits, in proportion as they approach the period of maturation, that the partitions, at first thick and opaque, terminate by becoming much swelled, transparent, and at last burst, to set at liberty the acid liquid which they contain.

We believe then that we may safely say, *that the sugar of the cane is always a primitive production*; whilst the sugar of raisins is always a secondary product. These facts are amply sufficient to explain how it is that the sugar of raisins is artificially formed with such great facility, whilst we have not been able to the present day to make the sugar of cane of whole pieces.



rived from the lower extremity of the plant, after having been treated with cold alcohol, at  $95^{\circ}$ , which dissolved it, was produced  $\frac{3}{100}$  of a deliquescent matter, either isolated from the matter  $\frac{8}{1000}$ , the residue was a deliquescent substance, soluble in water, neither sucroseous nor saline, and which left no ashes after incineration. To this treatment by cold alcohol, repeated three times, succeeded several experiments with boiling alcohol. After its reaction on the powder of the cane, the alcohol, by a slow cooling, left deposited some crystals of sugar, perfectly white and transparent; the liquid on its evaporation afterwards gave no trace of crystallizable sugar.

The same experiments were repeated with powdered cane taken from the summit of the cane; treated with cold alcohol, it gave out no liquid sugar, but a dissolution of a quantity of deliquescent matter sensibly the same as that obtained by the treatment of lower part; with boiling

This experiment which we regard as decisive, because it enabled us to exhaust the cane of its sugar, and to obtain it afterwards in small white crystals, has been applied to more than ten different specimens, taken from all parts of the sugar cane. If these experiments which we have just described, on the dried sugar cane, have not given us in any instance the least trace of liquid sugar (molasses) it has not been the same when we have treated the fresh sugar cane or its juice with alcohol.

In 100 grammes of the juice of the cane of a density of 108 ( $12^{\circ}$  Baumi) produced by expression, we have poured 50 grammes of alcohol at  $95^{\circ}$ , which gave out a flocculent precipitate, which was separated by the aid of a very fine filter. The alcoholic sucroseous liquid was placed under a large pan containing quick lime, at the end of 15 days all the water and a great part of the alcohol had disappeared, the sugar had disposed itself on the sides of the capsule, in whitish nipple like forms hard and brittle, it weighed 16gr. 8. The remaining alcohol, decanted and evaporated, left 2gr. 2. of a sucroseous, saline, deliquescent matter; ether isolated from it a very small quantity of wax; the rest, very soluble in water, obstinately refused to crystallize, or to give out to any menstrum, the sugar which it retained. alcohol no other matter was obtained but crystallizable sugar. By the direct treatment of fresh canes with alcohol, the evaporation in vacuo, of the burnt juice of the cane have given us the same result: crystallization has never been

complete, there always remained a residual sugary saline\* matter, the weight of which corresponded with the quantity of soluble salts contained in the canes which we analyzed.

We believe then that we may safely assert, first—That there is no uncrystalizable sugar pre-existing in the sugar cane. Secondly—That the salts react on the crystallizable sugar, in such a manner as produce molasses, that their action does not take place during vegetation; but depends much upon the manipulations; it cannot be otherwise, since in the vital act of vegetation, the different materials are elaborated in a complete independence of each other.

But what are we to think of the non-sacreous deliquescent matter which was obtained from the dried cane by alcohol at 95°?

We are inclined to believe that in the great work of vegetation, it has power to perform an important part in the production of the moloss, that is to say, that if it does not combine with the sugar, it naturally tends to augment the viscosity of the syrups, and to agglutinate the crystals in the latter crystallizations in the parent stem.

This matter is colourless when it has been obtained by evaporation in vacuo, or at a low temperature; it is coloured by heat: its aqueous solution presents these two distinct characters, of being precipitated by tanner's bark, and of being absorbed by pure animal carbon in such a manner that it cannot be lifted up again by cold water. We have seen farther, that the burnt juice of the sugar cane certainly proves the alcoholic fermentation; that it passes, with much greater facility, the viscuous fermentation, whilst that the alcoholic fermentation is always developed in the juice of the cane passed through animal black, which never follows the viscous fermentation. We have found this matter small in quantity in canes in our possession, but it cannot exist in a greater proportion. The canes called *folles*, are improper to furnish crystallizable sugar; such are the canes grown on new land, which take a much greater development. For the remainder we have no need to have recourse to hypothesis, in order to comprehend why it is that the juices of the cane are nearly uncrystalizable.

These canes have grown up on virgin lands, on those lands of which the trees which covered them have been

\* We have already indicated that the ~~cane~~ which we used in our experiments contained a much more considerable quantity of salts than the colonial canes of which we have made the analysis.

destroyed by the action of fire; a part of the soluble salts, in such great abundance in their fertile soils, have been carried along into the cane, exert in a more matured state of the cane, with a fatal influence, their chemical action on the crystallizable sugar.\*

The sugar juices always contain quantities more or less great of salts, and we know that sea salt combined with the sugar of the cane, gives birth to a delequiescent composition, which contains six parts of sugar for one part of sea salt, and which remains in the parent juice in the state of uncrystallizable sugar. But the chloride of sodium is not the only salt which can combine with sugar, and exercise a deleterious influence on the process of crystallization; we have observed that a great number of salts are often opposed to the crystallization of a baking syrup. The haloid salts may be placed in the first rank, the carbonates of potash and of soda react even on sugar at the baking temperature, and render the syrups high coloured and almost uncrystallizable.

The parent juices of the sugar cane, and the sugar beet, can yield molasses then, without these plants necessarily containing them during vegetation.

Let us remark, however, that a liquid never crystallizes without leaving a residue representing a solution at its point of saturation, and we will explain how M. Avequin obtained so much molasses, that is to say a crystallizable sugary juice, whilst M. Plagne, by crystallizing the juice four and five times, obtained an almost complete crystallization, a circumstance which is equally inherent to the mode of manipulation in the colonies.

A practical fact now comes to the support of what we have advanced: they then employ in the preparation of double-refined sugar the very finest sugar land; the syrup which is taken to be baked in vacuo is perfectly colourless and may be considered as a solution of sugar almost pure. During the first crystallization there is not more than 50 per cent. of crystallizable sugar, and it cannot be withdrawn farther without candying (*caraméliser*) a portion of sugar; this depends, according to the observation of M. Dubrunfaut, on the fact that a part of the water on boiling can not dissolve more than five parts of sugar, that

\* In the same manner as the sugar cane—The sugar beet grown in lands over manured, and rich in deliquescent salts, furnish syrups which crystallize with much greater difficulty, and furnish more molasses.

it abandons three only by cooling, whilst one part of cold water dissolves two of sugar, it is this dissolution which constitutes the parent syrup.

The cooling of a saturated solution of sugar from heat leaves then a parent syrup (*can-mere*) which is sometimes designated by the name of molasses, and which is found to be formed in reality of two parts of sugar and one of water, but still we are not ignorant that these molasses are far from being liquid or uncrystallizable sugar; we have even signalized this fact in order to explain why the Creoles who do not repeat the boiling, at least very rarely, of their running syrups, carry so high the figure of their molasses. It is true that those amongst them who do reboil them, remove from them in general the bad products only. The inferiority of these sugars of syrups holds so universally to this that in place of reboiling them each day, as in the sugar bakehouses, or refining houses of sugar in France, the planters wait so long before reboiling them, that by this means the crystallizable sugar, which these syrups contain in such great abundance, are found partially altered by a slow fermentation, and by the reaction of saline matters. A better understood treatment of this product, so rich in crystallizable sugar, would give, without contradiction, great benefits to the Creoles. In fact, on the place of manufacture, the rough sugar is worth from 50 to 60 centimes the kilogramme, whilst the molasses, whatever be their richness in crystallizable sugar, are never sold for more than 10 centimes the litre (7 or 8 centimes the kilogramme). On a great number of plantations, the molasses are almost entirely for the nourishment of the flocks; on others they are employed in the *qu'il diveries* in the extraction of rum; in France likewise the molasses are utilized for the nourishment of animals, and for the extraction of alcohol, but these are the molasses marked from 44 to 50° of the areometer of Baumé, whilst the molasses of the colonies are ordinarily marked from 37 to 38°. This difference in the density of syrups may vary from 6 to 50 per cent. the quantity of uncrystallizable matter in a running syrup. The planters then are able to obtain from their molasses 35 or 40 per cent. of sugar.

Let us prove that the perfected equipages for the process of baking produce the finest sugars, from syrups the richest in crystallizable sugar; but they will not augment the quantity of baked sugar obtained from the

first casting; let us also repeat, that it will always be necessary to have recourse again to the *rebaking of the syrups*, in order to obtain from them the maximum proportion of crystallizable sugar, as the practice of the manufacturer can teach.

These reflections, on the richness in crystallizable sugar, of a produce so precious and so badly utilised, have appeared to us to be worthy of being depended upon, at this time, when the planters are making all their efforts to progress in the art—whilst they avail themselves daily of the great improvements in their processes of manufacture—and whilst they acknowledge that they have too long been ignorant, or misunderstood the fertility of their soil, and the value of its products. New ideas are spreading each day in our islands; the Creole no longer has faith in the old modes of proceeding; the planters, with new kindled zeal, and animated with the love of good, like Messieurs de Jabrund and de Longchamps, adopt and encourage the most rational systems of manufacture; it is within the course of a few days that Havre saw depart from its shores, for Gondaloupe, a considerable number of baking equipages of the newest construction, amongst others, some constructed for M. de Jabrun, who is now returning to his plantations, after a deligation of six years; the same vessel likewise took out M. Hotessier, the author of an excellent pamphlet on improvements adapted to the manufacture of sugar from the cane. M. Hotessier has added to his pamphlet the description of some new baking apparatus, almost similar to those of M. de Jabrun, which apparatus present the most favourable conditions to a good fabrication.

During a period of some years numerous plantations have also availed themselves of improved apparatus; the impulse has been given, the Creoles will not leave, for a long time again, *thirty per cent. of sugar in their dry canes*; assuredly they will not, very soon, any more make forty per cent. of molasses, or even sixty, as is done in the isle of Bourbon. The metropolis, as well as the colonies, will soon feel the benefit of it, in the means of reconciling of from forty to fifty per cent. of crystal sugar.

The process of dessication of the cane, as at present applied on a grand scale on the island of Gondaloupe, gives rise to the brightest anticipations. Our works on the dried cane have proved, that dessication of them does not alter, in the least degree, the crystallizable sugar—

that the exhausting of the cane, the extraction of sugar only becomes more easy and more simple. That day, wherein the system of dessication shall present itself in its great superiority for the perfect exhaustion of the cane—that day, the planter, in place of renouncing the fabrication of sugar—in place of conceding this advantage to the metropolis, will work on his plantations his dried canes, and will employ his improved baking equipage for the evaporation of the syrups enclosed in the dried canes; and this would not hinder our great manufacturers and merchants from importing dried canes, in order to extract from them, in the refineries of France, immense products of sugar.

The general good—the interests of our colonies, have equally a claim to an augmentation in the production of sugar. That product—primary necessity—which its present dearness puts out of the power of the working class to procure. Its consumption, which seems to diminish, in lieu of relatively augmenting by ready money, hinders its use, and always places this salutary nourishment at a price inaccessible to twelve millions of the French nation. That, then, will be a bright day for our colonies, wherein the planter, doubling his produce by his industry, shall augment the cargoes of his vessels sailing for the metropolis—and would thus also give life and vigour to our merchant fleets.

We cannot perhaps give a better idea of this question, and the important part it will perform in our industry, than in citing some words of a memoir by M. Millott on the question of sugars.

“Nature has even ordained that sugar occupies the first rank in the vegetable order of which it is one of those products the most precious for aliment, as well as in a commercial point of view.”

“Sugar is so much an article of merchandise *par excellence*, that if the consumption of sugar is increased by a reduction of the price of this object of predelection, the cargoes of our merchantmen are increased by the carriage of the number of tons of sugar consumed.”

(To be concluded in our next Number.)

ART. V. - *On the means of detecting Arsenic in the Animal Body, and of counteracting its Effects*; by J. LAWRENCE SMITH, M. D., of Charleston, S. C.\*

*Messrs. Editors*—This, I hope will receive a place upon the pages of your Journal, if it be only for the importance of the subject of which it treats, although it is not improbable, in stating what I am about to do concerning the more recent experiments upon arsenious acid, that your readers will be able to find something which may be of importance to them in future investigations upon this substance. But two months have elapsed since the whole of France was agitated by one of the most interesting criminal processes upon record—it was a case of poisoning by arsenic; and the contradiction of the results of the medico-legal examinations, created an excitement which the decision of the jury augmented. Three chemical examinations were made upon different portions of the body, and at different times, to ascertain whether arsenic had been administered to the individual during life. The materials for the first were furnished immediately after death, and consisted of the fluid found in the stomach, the stomach itself, and a portion of the intestines; but the first was lost by an accident which happened to it while being experimented upon, so that the stomach and intestines alone remained. The second and third were made upon portions of the body exhumed after eight months' burial; they were the liver, heart, brain, and inner muscles of the thigh. The first and second examinations were made by several expert chemists of Tulle, without detecting the poison. The third M. Orfila was called upon to make, and he succeeded in exhibiting the metal, reduced by means of Marsh's apparatus; his success was no doubt owing to the manner in which he carbonized the animal matter, which was by the aid of nitric acid.

One cannot be surprised at the excitement that a thing of this character must have produced, and it is with much interest and benefit that I have followed up the chemical researches consequently arising, as well as the many interesting questions proposed for solution, and my object now is to mention the most important of them. Some of the questions are as follows:

- 1st. Does the hydrated peroxide of iron contain arsenic?
- 2d. Does arsenic exist normally in the animal tissues?
- 3d. Is not Marsh's apparatus subject to serious objections?
- 4th. What are the best means not only of detecting but of ascertaining the quantity of arsenic when in combination with animal matter?
- 5th. What are the best means of combating the poisonous effects of arsenic acid?

\* Silliman's Phil. Journal, for April, 1841.

To all these questions such answers will be given as have hitherto been furnished.

*Does arsenic exist in the peroxide of iron ?*

This question originated from the fact that this substance had been administered in the case spoken of; and there are those who suppose that the arsenic detected belonged originally to the peroxide of iron used as an antidote.

It is well known that arsenic exists in a state of combination in many of the sulphurets of iron, from which the sulphate is obtained, and it is the latter that furnishes the peroxide either by precipitation or heat. Both forms of this oxide have been subjected to minute examination by M. Orfila, who was particularly interested in this question, and the following are his experiments with their results :

" 1st. I boiled during four hours, in five capsules, four and a half ounces of hydrated peroxide of iron, taken from different apothecaries, with four ounces of distilled water, and by Marsh's apparatus no trace of arsenic could be obtained.

" 2d. I then added thirty grains of pure caustic potash to the hydrated peroxide of iron in each capsule, but no trace of arsenic could be obtained,

" 3d. But on treating by an ebullition of five hours an equal quantity of hydrated peroxide of iron in pure sulphuric acid, the liquid of three capsules out of the five gave arsenical taches.

" 4th. Four portions of four ounces each of colcothar of commerce, (the anhydrous peroxide of iron formed by heating the sulphate,) obtained from different merchants, by ebullition for four hours in distilled water, did not give indications of the presence of arsenic.

" 5th. This substance in the same quantity by ebullition during five hours with strong sulphuric acid, gave large arsenical stains with the aid of Marsh's apparatus.

" 6th. Thirty grains of colcothar boiled with sulphuric acid, gave arsenical stains.

" 7th. Fifteen grains of the same body, treated in the same way, gave no indications of arsenic.

" 8th. A solution of sulphate of iron gave no arsenical stains with the apparatus."

M. Orfila next administered four ounces of colcothar to three dogs, tying the œsophagus to prevent vomiting. One of them was examined in thirty-four hours after, the second fifty, and the third sixty. The liver, spleen, heart, and kidneys of these animals, were submitted to investigation, but no trace of arsenic could be obtained. The liquid of the stomach and intestines of the first dog being separated from the colcothar, gave arsenical stains though its urine did not indicate the presence of this metal. The



intestinal liquid of the second dog gave some stains, less apparent and less numerous than that of the third, but on the contrary its urine gave strong indications of arsenic.

The conclusions to be arrived at from these experiments are, that the hydrated peroxide of iron, and the colcothar, the former of which is administered as an antidote for arsenious acid, contain arsenic in minute quantities, (though the former being no doubt as often without as with it,) but that it requires the aid of a strong acid to develop it, and also, that when these substances are administered, the arsenic that they contain is slowly absorbed, passes by the organs, and is eliminated by the urine. The organs never at any time retain sufficient arsenic to exhibit it when examined for.

This question being answered in the affirmative, would appear to throw a great obstacle in the way of pronouncing with certainty whether the arsenic found in the intestinal liquid of an individual supposed to have been poisoned, and to whom the hydrated peroxide of iron had been administered as an antidote, was due to arsenious acid or to the oxide of iron. This difficulty would not arise except the quantity found be extremely small; for the peroxide of iron, from the manner in which it is prepared, can contain but the smallest appreciable amount; and, moreover, as it has already been remarked, it is not always that we find even that. The plan that the medico-jurist should adopt, in a case of this character, would be to examine the peroxide of iron that the person had taken, should there be any of it remaining, if not the sulphate of iron from which it was made. Again, he should lay but little stress upon the examination of the intestinal liquid, but direct his attention particularly to the organs. This, together with circumstances peculiar to each case, will explain away any doubt that might arise.

It ought to be perfectly understood, that the facts of the peroxide of iron containing a small quantity of arsenic, should be considered rather as a light to guide the chemist in his researches, than as a stumbling-block that might cause him to fall into error.

*Does arsenic exist normally in the animal tissues?*

This perhaps has been one of the most interesting questions ever proposed to chemists, and the investigations that it has given rise to, serve to show the almost perfection of their science, for were it supposed that the whole animal frame contained but one fiftieth of a grain of arsenic, the chemist would not despair not only of being able to detect it, but also of fixing its locality.

As it regards the bones, it has been clearly demonstrated that they contain arsenic in a minute quantity, but sufficient to place the fact beyond the smallest doubt.

Whether it exists in the muscles or not, it is a question by not

means settled. It is true, that with the aid of Marsh's apparatus there can be obtained from muscles digested a long while in nitric acid, stains which are of different shades, such as brilliant white, brilliant yellow, and rusty colour; they are volatile and not soluble in nitric acid. Many have supposed their composition to be sulphur with an infinitely small quantity of arsenic. I think that these stains can be more easily accounted for by sulphur and phosphorus, both of which exist in the muscles, and I am sorry that there is neither time nor opportunity to examine into the truth of this supposition. Nevertheless, whether they contain arsenic or not, the stains obtained have but one characteristic belonging to that of arsenic, volatility.

The next part of this question is very important; it is whether the organs, such as the liver, spleen, heart, &c., contain normal arsenic. The reason of its importance is, that it is upon them that we should place considerable reliance, in the examination of the body of a person supposed to have been poisoned by arsenic. To this we answer, that not the smallest trace has been detected in any of them; and the answer is based, not upon the few experiments of a single individual, but drawn from numerous careful researches, made by skillful chemists. What is still more convincing on this point, is, that even in some few cases, where an animal has been poisoned by arsenic, its liver will not indicate its presence.

To sum up the answer to this question in a few words—the bones do contain arsenic. No positive evidence has yet been given to lead us to believe that the muscles contain the smallest quantity of arsenic. We have the most positive evidence that the organs do not contain the least trace of arsenic.

*Is not Marsh's apparatus subject to many and serious objections?*

This valuable instrument I think was discussed a year or two since, by Dr. Mitchell, of Philadelphia, but as I have never seen his article on the subject, I hope, that if this should meet his eye, he will excuse such parts of it as may be a repetition of what he then stated. Most that is about to be mentioned concerning this apparatus, belongs to the investigation of those more intimately connected with the subject than myself.

Marsh's apparatus, modified from its original and rather complex form,\* consists of a four or eight ounce phial, with a perforated cork and glass tube, bent at right angles, or straight, (the former is considered preferable, though in both instances the extremity must be drawn out to a capillary opening,) and furnished with a porcelain plate or saucer, and the materials for generating hydrogen—zinc, sulphuric acid and water. These three last substances in effect constitute the instrument. The first question to be decided is, whether any of them are subject to an impurity that

\* See "Annals;" Vol. 1.

might create an error.

As regards the zinc, that there are some instances of the zinc of commerce containing a small quantity of arsenic, is not to be denied; and that this will give rise to an impure hydrogen, when acted upon by pure sulphuric acid and water. But then, again, there is nothing more easy than to procure zinc of commerce which will generate hydrogen perfectly free from arsenic, notwithstanding there are some who say that purified zinc is not free from this metal; but it is evident that they must be mistaken, as any one may see by making the experiment, which, as it is a very simple one, it would be well to perform; and I feel confident in saying that little or no difficulty will be found in procuring ordinary zinc of the necessary purity to be used in Marsh's apparatus.

Sulphuric acid may contain arsenic, when manufactured with sulphur obtained from pyrites holding that substance in combination; but a simple distillation will serve to rid it of this impurity.

After placing the zinc, sulphuric acid and water in the apparatus, replace the cork with the glass tube inserted in it, and when the hydrogen has been allowed to generate a sufficient length of time to expel the air, inflame it as it issues from the extremity of the tube; if a porcelain plate be now applied to about the middle of the flame, and no stain or spot be obtained, we have the best evidence of the purity of our materials.

Another apparent objection to the apparatus, is, that the introduction of animal matter, either solid or liquid, causes the formation of a large quantity of froth, which arrests the progress of the operation. This, however, is so easily remedied, that it need hardly be considered an objection. If the froth be not in too great quantity, it will suffice to introduce a little oil, which will serve to arrest its formation. Another method is to turn the liquid out of the phial into a funnel, with the finger placed upon the lower extremity, the froth will at once rise to the surface, and by taking away the finger the liquid will pass out perfectly free from it. Again, if care be taken to carbonise the matter before using it, this obstacle will be removed. There is still another means, and I find it to succeed very well in most instances; it is to pour the sulphuric acid destined for the formation of the hydrogen first upon the animal matter, and then pour the two upon the zinc and water; it would appear that a partial carbonization takes place. No doubt most persons will now perceive that this objection possesses no weight, and vanishes altogether before the means proposed to encounter it.

The next part of this question to be examined, is, what substances besides arsenic produce stains with this apparatus, and is there no danger of confounding them with that of arsenic? They are antimony, sulphur, phosphorus, and iron. Before speaking of

their distinguishing characteristics, it would be as well to say a few words concerning that produced from arsenic.

The arsenical stain is highly metallic, of a steel colour, with a slight reddish tinge, and borders of a dark rusty color; but to have a proper idea of its appearance, as well as that of the others, one should see them. It is volatile by heat, and is dissolved by cold nitric acid, which solution gives to the nitrate of silver a brick-red precipitate, the arsenate of silver.

The antimonial stain is less metallic than the former in its appearance, also blacker, and when very dense even smutty. It can be volatilized, but with great difficulty, and not before it has been as it were chased about the surface of the porcelain. It is soluble in cold nitric acid, which solution gives no red precipitate with nitrate of silver.

The next stain to be spoken of, is the compound one, of arsenic and antimony; at the same time mention will be made of the method adopted by M. Orfila for detecting the one or the other of these metals in it. It partakes, as might be expected, of the characters of both the metals that enter into its composition, being partially volatile, soluble in nitric acid, from which the brick-red precipitate of arsenate of silver can be obtained. M. Orfila proposes a plan of separating the constituents of this stain, and of testing each by itself. He proceeds as follows: having collected a number of the compound stains upon a porcelain plate, he dissolves them in nitric acid, which solution being poured into a capsule, is evaporated to dryness, and a residue remains composed of antimonious acid and a mixture of arsenic and arsenious acids. Upon this residue a little water is poured, and the capsule slightly heated, which enables the water to dissolve more readily the two last mentioned acids. The antimonious acid being allowed to settle, the clear liquid is decanted, and a few drops of nitrate of silver being thrown upon it, the brick-red arsenate of silver is formed, which is sometimes mixed with a considerable quantity of a yellow precipitate, the arsenite of silver. This will, however, rarely happen, if a large quantity of nitric acid has been used; for by so doing, only an extremely small quantity can remain in the state of arsenious acid, the oxidation being carried a degree higher. Nevertheless, if the entire precipitate produced by the nitrate of silver be yellow, it can have no effect in destroying the fact concerning the presence of arsenic, as it only indicates that it has met with arsenious and not arsenic acid. But to return to the substance left in the capsule:—A small quantity of muriatic acid, slightly diluted, is poured upon it, which immediately dissolves it. A current of sulphuretted hydrogen is now made to pass through this solution, when the orange-colored sulphuret of antimony is formed. Another process will be stated for arriving at the same

end, when mention is made of a method by which I propose to separate arsenic from organic substances. The importance of studying this double tache will be evident to every reflecting mind, for it may not unfrequently happen that the physician called upon to administer to a person supposed to be laboring under the effects of arsenic, may use tartar emetic to disembarass the stomach of the supposed poison; death taking place, an examination is made of the liquid found in the stomach and intestines, of urine, &c., by means of Marsh's apparatus, and a stain is obtained which is not easily volatilized, and which has the appearance of antimony. What then is to be done? Why, we are to proceed in our experiments as just stated, and the two metals, if both be present, are to be separated.

The stain from sulphur has all the characteristics of that substance; colour yellow, volatile, with a suffocating smell, &c. There is not the least probability of confounding it with any thing else.

The stain from phosphorus possesses three different shades, brilliant white, brilliant yellow, and rust color. When the quantity of phosphorus is very small, either the first, or only the first and second are seen. It is volatile, reddens litmus paper, and is insoluble in cold nitric acid, so that there cannot be the least occasion for mistaking between this and arsenic.

The next substance that produces a stain when introduced into the apparatus in question, is iron, but it ought not to be classed with the others, for I am firmly convinced that it is not due to any iron that may be dissolved by the hydrogen; in other words that there is no ferruginous hydrogen. My reason for so believing is based upon the following facts:—If we desire to obtain this stain, a considerable quantity of iron, or some salt of iron, must be used, and the gas made to generate rapidly. Now observe what must take place. The action of the liquid being violent, a spray is formed, which consists of the dilute acid and whatever salts it may hold in solution, in this case iron as one; this spray passes along with the hydrogen through the jet; the hydrogen being now ignited, a porcelain surface is placed in contact with the flame, which, becoming heated, enables it to evaporate the water from the salt of iron, which deposits itself, and afterwards becomes decomposed by a continuation of the heat, the peroxide being left. If we still retain the iron in the apparatus, but make the action not very brisk, no stain will exhibit itself upon a smooth porcelain surface; but if the broken surface of a piece of porcelain is placed in contact with the flame, a slight black deposit is formed, consisting, as in the former case, of peroxide of iron; the reason of this is, that it is a more convenient surface for retaining the particles of the solution of iron thrown out in company with the hydrogen. Again, this stain is evidently an oxide, which it is not probable

would be the case, had the iron been chemically combined with the hydrogen. Another reason is, that if the gas be made to traverse water or chloride of calcium before ignition, no stain will be formed, for the iron mixed with the hydrogen is retained by either of these means. This stain has been perhaps more noticed than it deserves. It is not easily produced, and is distinguished by its not being volatile and its solution in any of the strong acids, giving a blue precipitate with ferrocyanuret of potassium.

There is yet one other stain to be spoken of. If the flame of the apparatus, containing only zinc, sulphuric acid of water, be prolonged for some time upon one spot on the porcelain, an opaque white stain will be perceived, which I propose to explain in the same way as the last, the cause of it being the oxide of zinc instead of iron, this oxide arises from the decomposition of a small quantity of sulphate of zinc thrown out with the hydrogen, but still it is a thing hardly worthy of notice, for after it is formed it is difficult to see it.

What is the conclusion to be arrived at concerning Marsh's apparatus, after what has been said? Why, that it should be considered as the most valuable instrument that the medico-jurist possesses, to assist him in his experiments upon the poison in question; for with proper care all the objections to it can be easily remedied, and the character of each stain is so well marked that they need never be confounded, as will be seen by glancing the eye over what follows.

*Arsenic*—Steel colour, highly metallic, easily volatilized by heat, readily dissolved in nitric acid; the nitric acid solution gives with nitrate of silver a brick-red precipitate.

*Antimony*—Colour darker than steel, metallic, with difficulty volatilized by heat, readily dissolved in nitric acid; the nitric acid solution gives with nitrate of silver no precipitate.

*Sulphur*—Colour sulphur-yellow, easily volatilized by heat, not soluble in nitric acid, gives the well known smell of sulphur when burnt.

*Phosphorus*—Colour brilliant from white to red, easily volatilized by heat, not soluble in nitric acid, reddens litmus paper.

*Iron*—Colour black but slightly metallic, not volatilized by heat, soluble in nitric acid; the nitric solution strikes a blue colour with ferrocyanuret of potassium.

#### *Examinations for arsenic in case of poisoning.*

Under this head will be answered the fourth question, which is, What are the best means not only of detecting, but of ascertaining the quantity of arsenic in combination with animal matter?

Arsenious acid, it is well known, does not destroy life by a mere local action upon the stomach and intestines, as do many of the strong acids, but that its poisonous effects are exhibited after it

has been absorbed into the system. It is true that it inflames the mucous membrane of the intestinal canal, but that is comparatively of minor importance to its other effects. If it be absorbed, in what secretions and in what organs is it to be found in the greatest abundance? The urine is the first secretion in which arsenious acid exhibits itself, and in that not long after administration. This fact, then, makes it important to preserve the urine of a person who we may suppose has been poisoned by this agent, for making the necessary medico-legal examination, and in cases where death does not occur it ought to be considered of more value than the matter vomited.

After the bladder, the liver and heart next demand our attention, for one may calculate with almost absolute certainty upon finding this substance in these organs, had it been employed. The brain and inner muscles of the thigh, in most cases of poisoning by arsenious acid, contain it in sufficient quantity to be exhibited by means of Marsh's apparatus. Other portions of the body frequently contain it in small quantities, but if we have the organs already mentioned, along with the stomach, intestines, and their contents, it will be all that it is important to experiment upon.

In commencing the experiments we should be furnished with the following materials, viz. nitric and sulphuric acids, nitrate of potash, zinc and water. Their purity should be fully established before they are employed.

The use of the nitric acid is to carbonize the animal matter, and in that way to develop any arsenic that it may contain. This process is of vast importance, as will be seen by the following example. Let the liver contain the largest quantity of arsenious acid that can reach it by the process of absorption, and it may be boiled for six hours, in distilled water, without giving up the smallest portion of the poison; whereas, carbonize it first by the aid of nitric acid, and then pour the water upon it, and results of an entirely different nature will be obtained. There are no doubt, two reasons for the cause of this; the first is, that the arsenious acid has undergone some chemical change, which renders it insoluble; the second is, that the liver is completely broken up by the nitric acid, and the arsenic, in whatever state it may have existed, is now converted into arsenic acid. The nitrate of potash is sometimes employed to destroy the carbon after the nitric acid has acted upon the animal matter. The sulphuric acid, zinc and water, are the elements of Marsh's apparatus.

The fluid of the stomach and intestines should be first experimented upon; and this may be introduced into the apparatus either in its crude state, or after having undergone carbonization by heat or nitric acid. If it be employed uncarbonized, we may expect a great quantity of froth, which may be obviated in some

measure by the means already mentioned. When we carbonize the matter by heat, it becomes necessary to introduce a small portion of pure caustic potash during its exaporation, which combines with arsenious acid, forming arsenite of potash, a substance not easily volatilized. If nitric acid be used, we first evaporate the liquid to dryness, then pour upon it two or three times its bulk of nitric acid, and again evaporate to dryness, when we may expect an almost complete destruction of the animal substances. The carbonized matter, formed either by heat or nitric acid, with whatever it may contain, is digested for a little while in pure water, which easily dissolves the arsenic, now in the states of arsenite of potash, and arsenic acid. Filter, introduce the liquid into the apparatus, when we may expect to exhibit the metal upon a porcelain surface. In experimenting upon the urine, the same steps are to be taken.

The examination of the liver is conducted as follows:—Two or three pounds of it are first dried by a gentle heat, and then digested with about three times as much nitric acid by weight, until the mass becomes perfectly dry; water is now poured upon it, and heat applied for ten or fifteen minutes; the liquid is now filtered, and tested by the apparatus. The heart muscles, brain, &c., if examined, must undergo the same process.

There is yet another advantage, that has not been mentioned, connected with the carbonization of animal matter by nitric acid; it is, that if antimony be present, it becomes converted into antimonious acid, which is insoluble in water.

Mention has been made only of the manner of separating arsenic from animal matter, by the aid of Marsh's apparatus, and it may be well to give a brief account of one or two new methods adopted by Mr. Persoz to serve the same end, with this additional advantage, that it enables one to ascertain the exact amount present.

The suspected materials, after having sufficient reason to suppose that they do not contain a poison of organic origin, or mercurial or antimonial preparations, are subjected to the action of dilute nitric acid, in order to destroy those parts that are decomposed by this agent. Most of the organic substances having undergone this decomposition, the residue is diluted with water, and heated to the boiling point, and then left to cool; the fatty and resinous substances rise to the surface, are taken off and washed, and the washings added to the original liquid, which is then evaporated to the consistency of syrup. The liquid now has a dark brown tint, an evidence that it still contains a quantity of organic matter. Nitric acid, therefore, is again added, and a new oxidation takes place. We recommence to evaporate, and continue to add nitric acid, until the liquid acquires a lively orange tint, when a careful evaporation is commenced, first over a naked fire,



and then by the means of vapour. An approximate value being made of the quantity of residue, twice and a half times its volume of pure nitrate of potash is added, for the purpose of completing the oxidation. Water is next poured upon these materials, and heat applied and continued until the water is evaporated and the residue is dry; by this means an intimate mixture is brought about between the nitre and animal substances. In this part of the operation, care must be taken to extend the matter as much as possible over the surface of the capsule as soon as it begins to dry. The capsule is now heated almost to redness, when a deflagration takes place, and propagates itself through all the matter submitted to analysis, destroying all remains of organic matter. Care must be taken that the nitre be in sufficient quantity, for if not, this part of the process must be gone over a second time.

After the deflagration has taken place, it may be well to heat the residue a second time, in a capsule of platinum or silver, to redness. The residue consists generally of the following substances: the excess of nitrate of potash mixed with the nitrite of the same substance; carbonate of potash; the salts existing in the organic matter, as well as those formed during the process, such as the phosphates, sulphates, and chlorides, free oxides, and finally arsenic acid, free and in combination with potash. This compound mass being pulverized, is mixed with one and a half times its bulk of hydrochlorate of ammonia, introduced into a retort, and heated to a dull redness. By the action of the heat, the chlorine of the hydrochlorate of ammonia combines with the potassium, and the hydrogen of the ammonia reduces the arsenic acid to the state of arsenious acid, which sublimes with the excess of hydrochlorate of ammonia, and is condensed on the upper part and neck of the retort. Other chemical changes take place, but they do not modify the one just stated. The operation being finished, the retort is broken, and the substance sublimed dissolved in water strongly acidulated with hydrochloric acid, and through this solution is passed sulphuretted hydrogen, which enables us to obtain all the arsenic that was originally in combination with animal matter, in the state of a pure sulphuret. This process is somewhat complicated, but each step is so clear, that with proper care, the most satisfactory result might be obtained in almost all cases.

In fulfilling the promise as regards the stating of all important facts lately brought to light concerning this too universal poison, I will mention two other methods of separating and of ascertaining the quantity of arsenic in combination with organic substances. They are both modifications of Marsh's apparatus; one is proposed by M. Lassaigne, and the other by myself.

M. Lassaigne, instead of igniting the arsenuretted hydrogen, and obtaining the arsenic upon a cold surface, passes it through a

solution of nitrate of silver, which it has the property of decomposing. The solution first becomes brown, and then a deposit of oxide of silver takes place. After the gas has ceased to pass, a quantity of muriatic acid is poured upon it, which decomposes what nitrate of silver remains, and converts the precipitate into chloride of silver. There remains now in solution arsenic and arsenious acids, and by filtering or evaporating to dryness, they are obtained.

I propose to pass the arsenuretted hydrogen through a tolerably strong solution of iodine in alcohol, in order to decompose it, which it does effectually, there being formed the iodine of arsenic, which remains in solution. All that is now necessary to be done, is to evaporate nearly to dryness, until red fumes make their appearance, and then pour twice or thrice as much nitric acid as there is residue in the capsule. Heat is again applied, and the evaporation continued to dryness, when there will be remaining arsenic and arsenious acids. The nitric acid in this case converts the iodine of the iodide of arsenic, and the free iodine into iodic and iodous acids, both of which are evaporated with the undecomposed nitric acid.

Iodine also decomposes the antimoniacal hydrogen, first forming iodide of antimony, which the water of the alcohol immediately decomposes into hydriodic acid and oxide of antimony, the latter of which is precipitated. This then becomes a convenient mode of separating the two substances, antimony and arsenic, for by passing the compound gas through the alcoholic solution of iodine, it becomes decomposed, and iodide of arsenic is formed, which remains in solution, and the oxide of antimony which is precipitated, can be separated by means of a filter. This, however, is not the plan that I would propose; it would be better to invert the precipitate as well as the liquid into a capsule, evaporate and treat with nitric acid as in the case of arsenic, when we shall have left the arsenic, arsenious and antimonious acids, the two former of which are soluble in water.

One may now imagine that there is nothing easier for the medicolurist than to form a correct opinion, and one that cannot be doubted, concerning the poisoning by arsenic. Whether such is the fact or not, he will find, in some cases, that all his skill and care will be required, not only to convince the minds of others, but even his own. It may not unfrequently occur, that arsenious acid has been the poisoning agent, and still great difficulties present themselves, which are enumerated in almost all works on medical jurisprudence.

There is one very important fact to be kept in mind with reference to examinations of this character; it is the medical treatment that the individual has been subjected to before death. For

instance, the treatment by diuretics, which will be mentioned presently, may remove from one or more organs the poison previously contained in them, and still the impressions made upon them be too strong to be recovered from. How then is this difficulty to be removed? By carefully preserving all the urine—an observation which is of such importance that it should not escape the memory of any physician.

*What are the best means of treating the poisonous effects of arsenic?*

A few words upon two new methods of treating the effects of arsenic, will conclude this article, already extended much farther than I had intended.

The remedies that we already possess, are, at the very best, but feeble agents to combat the effects of this poison. The one most to be relied on is the hydrated peroxide of iron, it being a veritable antidote to poisoning by arsenic; however, there are some objections, the principal of which is the slowness of its absorption, for it is only where it encounters the poison that its salutary effects are displayed, by forming with it an inert arsenite of iron.

A treatment proposed in Italy, is the administration of stimulant draughts every two or three hours, consisting of brandy one ounce, wine two ounces, bouillon (the liquid produced by boiling beef or other meat in water) four ounces. It is based upon the supposition that the effects of arsenic are atonic, the truth of which is far from being established. Instances are given where this treatment has proved efficacious, although I have witnessed experiments made with it, in comparison with simply tepid water, where the latter proved to be the most successful of the two.

The treatment by diuretics is one that deserves some consideration; it is advanced by M. Orfila, based upon numerous experiments. It has been more than once stated, that the urine exhibits a large portion of the arsenious acid absorbed into the system, and it seems very rational to suppose, that if this secretion could be augmented by any means, that the quantity of arsenic carried off would be also increased. It has been observed, that where equal quantities of arsenious acid have been given to two dogs of equal vigour, and if one died and the other survived its effects, we find that the latter had urinated largely. The diuretics merit some attention; they are not to be used until the stomach is emptied of its contents by some mild emetic and tepid water.

In a medico-legal examination for antimony, most of the steps that have been proposed in the case of arsenic can be followed; the principal modification is where either nitric acid or heat has been used to carbonize the animal matter; for in that case, muriatic acid slightly diluted is to be employed as the dissolving agent instead of water.

Paris. December 6, 1840.

*The employment of Iodine as a reagent for Hydrosulphuric Acid ;*  
by M. ALPHONSE DU PASQUIER.\*

TO THE EDITORS OF THE AMERICAN JOURNAL OF SCIENCE.

*Gentlemen*,—THE original of this article was published in the March number of the *Annales de Chimie et de Physique*, and the importance of its being generally known to those who devote any of their time or attention to the investigation of our mineral waters, which are more or less impregnated with Hydrosulphuric acid and the alkaline hydrosulphates, has induced me to transmit to you, for publication, a translation of such parts as explain the method of employing the reagent in question, and the conclusions that M. Alphonse has arrived at by his varied experiments.

The sulphohydrometer that is described, is of easy application, and enables one to obtain very accurate results in a short space of time, particularly when use is made of a table that I have calculated and annexed.

As regards the strength of the tincture of iodine, that is altogether optional with the individual who employs it ; it being only requisite to have a knowledge of the amount of iodine contained in a measured portion of the liquid. I should propose, as most convenient, that each division on the sulphohydrometer should answer to  $\frac{1}{10}$  of a grain of iodine, and a subdivision to  $\frac{1}{100}$ .

Yours respectfully,  
Paris, Sept. 20, 1840.

J. LAWRENCE SMITH, M. D.

“ To determine the proportion of hydrosulphuric acid, either free or in combination in sulphureous waters, is an operation attended with considerable difficulty, and of which the results are far from being certain. All the methods employed to arrive at this end, comprising even the process of M. Grotthuz, (the employment of ammoniacal nitrate of silver,) adopted by M. Anglada, and the generality of chemists of the present day, present great difficulties of detail, and are, as has been demonstrated in my first memoir, subject to gross errors, particularly when we obtain a sulphuret more or less impure ; and moreover when the quantity of hydrosulphuric acid is very minute they cease to act.

“ In my researches upon the waters of Allevard, the uncertainty of these methods, made me desire to discover some process more satisfactory, when, employing as a reagent the alcoholic tincture of iodine, (it not being among those ordinarily used ;) I found that the decomposition of the hydrosulphuric acid by this metallloid, was complete and instantaneous, and that one could determine, in every easy manner, the precise point at which the decomposition of the

\* Silliman's Phil. Journal, for January, 1841.

hydrosulphuric acid is achieved, or when the iodine no longer enters into combination. I conclude, from this fact, that, with a tincture of which I know before hand the proportions, I shall be able to ascertain, by the quantity of iodine employed to saturate a litre of the sulphurous water, the precise amount of hydrosulphuric acid which it contains.

“ Moreover, I am able to ascertain the quantity of iodine employed, without the use of a balance, by the means of an instrument which I call a sulphohydrometer. This instrument is a graduated tube, which allows the tincture of iodine to flow from an elongated extremity with a capillary opening, the other extremity being closed by a stopper.

“ To employ the sulphohydrometer, we take a certain quantity of the sulphurous water which we may wish to analyze, and placing it in a porcelain capsule, add a few drops of a very clear solution of starch, and then allow the tincture of iodine to fall upon it, drop by drop, from the instrument, previously filled to the point marked 0°, and continue the addition so long as no change takes place in the colour of the water, favouring the reaction by agitation with a glass rod. So long as there remains the smallest trace of hydrosulphuric acid, the iodine disappears as fast as it is introduced, and the starch, upon which iodine in a state of combination does not act, gives rise to no colouration of the liquid until the hydrosulphuric acid is completely saturated, when the minutest addition of iodine at once strikes a blue colour with it. We then examine how many degrees of tincture have been employed, and knowing the strength of it, we are enabled to calculate the quantity of hydrosulphuric acid decomposed by it.

“ This method of analysis, independent of its affording results of the most accurate character, has the additional advantage of being executed in so short a space of time, that one may make from fifteen to twenty experiments in less than one hour, and at the same time be perfectly sure of committing no error. It is also so easily put into practice, that any physician or intelligent person may apply it, and assure themselves daily of the variation in the strength of the sulphurous waters caused either by atmospheric changes or an admixture with rain water.

“ The conclusions that I have arrived at by my experiments are as follow :—

“ 1st. That the best known reagents for hydrosulphuric acid are subject to great objections, since they do not indicate even notable quantities of the acid, free or combined ; a circumstance that explains why its presence has not been demonstrated in waters whose physical properties rank them as sulphureous.

“ 2d. That an alcoholic solution of iodine, employed along with starch, is a very sensible reagent for hydrosulphuric acid, free or

in a state of combination. It can detect, in an undoubted manner, (by a comparative examination with common water,) a drop of concentrated solution of any of the alkaline hydrosulphates, disseminated one hectolitre\* of water, although the known reagents lose their action when the same quantity is disseminated in only ten litres.†

“ 3d. That with the tincture of iodine and starch we can recognize infallibly, in the weakest sulphureous waters, in those where ordinary reagents are useless, not only the presence, but also the quantity of hydrosulphuric acid, either free or in a state of combination.

“ 4th. That the known processes for determining the proportion of hydrosulphuric acid, free or combined, are so long and difficult that their result is uncertain and incorrect, especially in regard to waters possessing but little of the sulphureous principle.”

*Table of the quantity of Hydrosulphuric Acid decomposed by quantities of Iodine from  $\frac{1}{100}$  to 10 grains.*

IODINE. Weight in grains.	HYDROSULPHURIC ACID.		IODINE. Weight in grains.	HYDROSULPHURIC ACID.	
	Weight in grains.	Bulk in cubic inches.		Weight in grains.	Bulk in cubic inches.
.01	.001351	.003691	.60	.08106	.22146
.02	.002702	.007382	.70	.09457	.25837
.03	.004053	.011073	.80	.10808	.29528
.04	.005404	.014764	.90	.12159	.33219
.05	.006755	.018455	1.00	.13510	.36910
.06	.008106	.022146	2.00	.27020	.73820
.07	.009457	.025837	3.00	.40530	1.10730
.08	.010808	.029528	4.00	.54040	1.47640
.09	.012159	.033219	5.00	.67550	1.84550
.10	.013510	.036910	6.00	.81060	2.21460
.20	.027030	.073820	7.00	.94570	2.58370
.30	.040530	.110730	8.00	1.08080	2.95280
.40	.054040	.147640	9.00	1.21590	3.32190
.50	.067550	.184550	10.00	1.35100	3.69100

\* Hectolitre, about 26½ gallons.

† Ten litres, about 2½ gallons.

*Description of a Double Exciting Cylinder Electrical Machine,  
in a Letter to the Editor.*

SIR,—In consequence of your invitation to the readers of your excellent Annals of Electricity, &c., to furnish you with any new or original plans of apparatus that may in any wise promote the interests of the science, I have taken the liberty of forwarding you the enclosed sketch (Fig. 6, Pl. I.) and description of one I contrived and made about ten years ago :—

When first made, it was upon *Nairne's* principle, with a parallel positive and negative conductor, both the same size, and each the length of the cylinder and two-thirds of its diameter, which was made out of the usual proportions for compactness, 12 inches long and 13 inches diameter ; there need be no fear about the fluid running off, if there is a sufficient number of clean and sharp points of brass wire running along the face of the cylinder from edge to edge, as in the sketch.

I thought it a great waste of time and labour to excite in one place only about 30 square inches of surface, out of nearly 450, upon each revolution of the cylinder, as is the case upon *Nairne's* plan ; and that there was nothing to prevent a greater amount of useful excitement, if the fluid generated could be effectually carried off to the prime conductor, which object I have attained, so as to double the power, or nearly so, of the machine, and give it a compact and better appearance, without adding much to the expense, which plan and advantages may be easily adapted to any old machine now in use.

The construction will be easily understood by reference to the sketch. The prime conductor is insulated by a stout glass pillar, cemented into a wooden socket that screws on to the top of the front pillar of the machine on the underside of the conductor ; rising inwards are sockets, into one of which a hollow arm, about 1 inch diameter fits, that extends across the top of the cylinder, fitted with a row of collecting points, each about an inch long, and apart, so as to extend to the edges of the cylinder. From the end of the arm nearest the conductor proceeds a piece of No. 1 brass wire, bent into the size and curve of the edge of the cylinder, or rather larger ; reaching round to the bottom of the cylinder, to which is attached another similar arm, to carry the fluid from the second cushion : the curved wire is bent at each end about 3 inches, so as to make the arms sit parallel to the cylinder, and they pass into a cork cemented into the ends of the arms, with a bit lipped out of the edge to receive the wire to keep them steady in their places : from the front end of the prime conductor proceeds another arm that also fits into a similar socket, at the bottom end of which is a wire with a ball to adjust to the proper height for charging jars, &c., as represented in the plan.

The frame and negative pillars are made of seasoned beechwood, baked

to complete dryness, and immediately worked up and coated with varnish, made of equal parts of gum mastic and shellac dissolved in alcohol, the pillars coated eight or ten times over as fast as they become dry and hard, which I find secures as perfect an insulation as glass when wiped clean and dry, with the advantage of strength and no risk of breakage. The conductors are made of sheet tin and gilt over with Dutch metal leaf, having one coat of the varnish over it to prevent their being discoloured by the action of the atmosphere, it does not destroy their efficiency, and gives them a smart appearance at little cost. The axis that the cylinder is mounted upon runs completely through the cylinder, and is about half the diameter of the holes it passes through, which allows of its being so wedged as to make it the truest on the surface for excitement; it is then secured in its place by wooden caps covering the projecting necks cemented to them and the end of the cylinder.

If you should deem this worthy of your notice in your Annals, and entertain any desire for other kinds of apparatus not commonly in use, I have another or two contrivances for useful purposes that I shall have great pleasure in submitting to your notice.—I am, Sir, your most obedient servant,

RICHARD EATON.

Muchpark Street, Coventry,  
April 30, 1841.

*Lightning Conductors.* By J. MURRAY, Esq., F.G.S., &c.

SIR,—As your columns seem ever open to communications which relate to the public well-being, or are connected with the question of individual benefit, I venture to trespass on your time, and to solicit a niche in your columns. Favouritism, I fear, mingles often in Parliamentary Committees and Government Commissions of Inquiry, and hence a one-sided and partial view of the question terminates the case. I had no reason to be disappointed in the very imperfect and useless investigation on the subject of shipwreck, consigned to a Committee appointed by Parliament, when Mr. Dennett was not examined, and his rockets, every way superior to the invention of Manby, formed no item in the recommendations of the Committee; but I think I have a right to complain in the case of the favouritism shown to Mr. Harris, of Plymouth, in the recommendation for adopting his conductor in the British Navy. The opinions of Faraday, Daniell, and Wheatstone, the personal friends of Mr. Harris, seem to have been a *ne plus ultra*, and were made to bear entirely and exclusively on his lightning rod, as if no other had been in existence, or ever proposed. It may be also reasonably asked why Mr. Crosse, of Broomfield, was not examined on the occasion—one who is perhaps more practically conversant with atmospherical electricity than any other?



My lightning conductors have been long before the public, and their efficiency fully proved in many a thunder-storm during the last twelve years, so that there is nothing hypothetical or problematical in reference to my invention. More than thirty of these conductors have been erected in Yorkshire alone. It has been described in my work on the subject, as well as in the "Annals of Electricity." I am not aware that Harris's conductor has ever been erected, and Mr. Roberts and Mr. Sturgeon have controverted its efficacy. Mr. Harris sues for their *gradual* introduction into the British Navy; but, if there be no doubts as to their complete efficiency, or any "compunctious visitings of nature," why not entirely and at once?

My lightning conductor comes before the public recommended for its *economy* and *simplicity*, while it stipulates *all the conditions* which should enter into the composition of a complete storm conductor, calculated on the sound principles of induction. Its perfect security has never been questioned, nor has any one ventured to doubt the absence of any of the elements of a good lightning rod. It may certainly be received as an axiom, that a bad or imperfect conductor is infinitely worse than no conductor at all.

The elements of a good conducting medium for lightning in the storm, are these:—A provision for its prompt and safe receipt—a continuous conducting medium for its subsequent transit, that being a material fraught with the best conducting character—and further, a safe escape to the subsoil, or to water. As lightning penetrates to a depth scarcely appreciable, the substance of good conductors, a considerable superficial extent is requisite.

I employ copper gas piping, screwed together in definite lengths, and terminating above in an elongated and pointed solid copper pyramid. Immediately beneath this terminus, the hollow stem is perforated, so that the lightning may also penetrate the interior. This interior surface may be preserved for ever free from oxidation. The continuous rod enters the earth, and terminates in a well, or the subsoil. In order to protect the external surface from oxidation, I apply the galvanic principle of strips of zinc, insulated by bands of leather, and connect the zinc with the copper stem by copper wire.

It is easy to see the application of this conductor to ships. The higher parts of the conductor may be made to slide into each other like the tubes of a telescope, to meet the contingencies of the topmast or topgallant mast being struck (lowered) in a storm. The lower end of the conductor passes through the keelston.—Yours, &c.,

May 18th, 1841.

J. MURRAY.

P. S.—Apart from the question of "the lateral explosion," I consider a *smooth rounded surface* essential to the character of a good conducting

medium; a conclusion, of course, at variance with Harris's strips of metal. This communication has been transmitted to, and its receipt acknowledged by, the Earl of Minto.

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## LECTURES ON ELECTRICITY, &c.

### LECTURE IX.

THE electrical light which was so brilliantly exhibited in some of our latest experiments in the last Lecture, is capable of being moulded into a great variety of very beautiful experiments, some of which I will presently show you. You have already seen that a spark taken from the prime conductor, is capable of penetrating and making its way through a stratum of air, of the usual density, more than twelve inches thick; and it will traverse a much thicker plate of air that is attenuated, because, when in the latter condition, the air is a much better conductor than when it is dense: but in proportion as we attenuate the air through which a current of electric sparks is passing, the light becomes more feeble, and that brilliancy which the electric fluid exhibits in air of the common density, is entirely lost in highly attenuated air.

There is an experiment, however, usually called the Falling Star Experiment, and of which I shall speak more particularly in a future Lecture, in which the light is very brilliant when the air is attenuated to a certain degree; but in that case we transmit a greater quantity of the electric fluid, in one spark, than can be accumulated on the surface of the prime conductor, for which purpose we employ an instrument called the Leyden jar.

A very simple piece of apparatus, for showing the electrical light in a pleasing manner, is made by a few leaden shot, (duck-shot, for instance) strung on to a thread of silk, keeping a space of about one-tenth of an inch between every two. Twenty or thirty, or even more, of these small globes, may be thus arranged, and the ends of the thread cut, or burnt, close to the extreme globes of the series, so that no loose ragged end may interfere with the experiment; then, by holding one of the extreme shot between the finger and thumb, and letting the rest hang down to the ball of the prime conductor, so that the lowest do not touch it, you will see a beautiful series of sparks traversing the silken thread from one end to the other.

In this experiment, it will be observed, the sparks appear between the leaden globes only, and no light is observable whilst the fluid traverses the balls; therefore, the lowest shot first receives the sparks from the prime conductor, and delivers them to the second, which again delivers them to the third, and so on, from one to the other, until they arrive at the hand, where they are lost.

This instrument, it is true, is but a very rude substitute for some of those elegant pieces of apparatus which are usually employed for the same purpose, but, at the same time, it answers all the purposes of explanation quite as efficiently as they do. The apparatus generally employed are pieces of glass, either flat, or in the shape of tubes, partially spangled with discs of tin foil. They are generally constructed very tastefully, and the spangles of foil suitably arranged, so as to diversify the route of the sparks in a variety of ways, which give to their display a more pleasing and interesting effect than could otherwise be produced.

The glass tubes are capped at both extremities, with hollow spheres of brass, well polished and lacquered, and the spangles of tin foil are arranged round and round the tube, in the form of a spiral, from one end to the other, taking care that no two of them touch one another. The spangles are attached firmly to the glass, by means of strong gum-water, and the whole occasionally covered with a coating of lac varnish. The spangled tube, however, is sometimes placed inside of another glass tube, which, when capped by brass spheres at both ends, excludes all dirt and moisture in future. This method of fitting up these spangled tubes is, therefore, much preferable to that of using one tube with varnish only. When one end of this apparatus is held in the hand, and the other presented to the ball of the conductor, a series of sparks are seen traversing its surface between the spangles, and, consequently, in the same spiral line.

Another form of apparatus for this purpose, is that of a long strip of plate glass, on one side of which an arrangement of metal spangles, from one end to the other, is fastened by means of gum-water, and the line of electric light displayed by the sparks, will vary with every variation in the arrangement of the spangles. The opposite side of the glass is usually varnished in bands of different colours, which vary the colour of the sparks accordingly with the colour of the medium through which they are seen. When a spiral arrangement of metallic spangles is placed on coloured glass tubes, such as blue, purple, violet, &c., the sparks will assume different colours as they proceed from one end to the other. They are perfectly white whilst passing along that side of the tube next to the spectator, but will assume the colour of the tube at those parts of its route, which are behind it. The varieties of arrangements, or routes which the electric fluid may be made to assume, are almost endless in this class of experiments: but I shall pursue them no farther until I have made you acquainted with some other interesting facts.

There are some curious circumstances connected with the electro-conduction of bodies, which I did not mention whilst classifying them in our second lecture. They are these. Those solid bodies which have been called non-conductors are considered to be such, only whilst in a state of solidity, or whilst their parts are in a certain state of aggregation. For

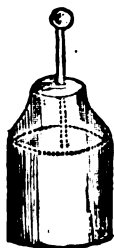
instance, glass whilst solid and perfectly dry, is a non-conductor; but melted glass is a very good conductor. Crystals of several salts also possess the same faculties; for although non-conductors whilst solid, they become conductors when in a state of fusion by heat. Green wood is a conductor of electricity, but baked wood is an insulator. If the baked wood be converted into charcoal, it is again an excellent conductor; but burn it to ashes and it loses the conducting faculty.

I will now show you a very satisfactory experiment on this point. You are already aware that a solid stick of sealing wax is a non-conductor; but if we warm the same stick, so as to make it quite soft, and then apply it to the cap of Bennet's Electroscope, whose leaves are already in a state of electro-divergency, you will soon find that the electric action disappears, and the gold leaves hang down close together. A tube of glass presents us with a similar fact, for although a non-conductor whilst cold, it becomes a conductor by being made to assume a high red heat; and better still when white hot.

By taking advantage of these facts, an illustrative experiment has been established, whose effect is called the *Electro-spinning of sealing wax*. I have a stout brass wire, which will just fit the hole in the end of the prime conductor of the machine. It is somewhat pointed at the other end, which is stuck into a short stick of soft sealing wax. When the wax has become hard and cold it is no conductor, but, as we have already seen, when melted, or even softened considerably, it becomes a conductor. I place this wire in the end of the prime conductor, and afterwards soften the wax by the flame of a spirit lamp; and now, whilst the machine is in operation, I hold a blank card in front of the softened sealing wax, and I find that a considerable quantity of the melted wax has been blown off from the wire, and is attached to the card in beautiful fine filaments, as soft and flexible as flocks of wool, or silk. By repeating this experiment with several cards, we have an opportunity of varying the arrangement of the fibrous wax in many ways. A great quantity may be collected upon the surface of one card, and placed in a cabinet, as a curious production of this species of electric action.

I will now introduce to your notice another piece of electrical apparatus, called the *Leyden jar*. It is represented by Fig. 10. It consists of a glass jar, sometimes perfectly cylindrical, and sometimes of the shape shown in the figure; but its shape is a mere matter of choice, for I shall have to show you very shortly, that a piece of flat window glass possesses the same powers as when in the shape of a jar, or of any vessel whatever; but the instrument is more frequently used in the shape of a jar, as in Fig. 10, than in any other.

Fig. 10.



The Leyden jar is lined with tinfoil from the bottom to within about two inches and a half of the top. Some persons have three or even four inches of the upper part of the jar uncovered with the foil. Its outside is also covered with tinfoil to the same height as the lining. The foil may be stuck to the surface of the glass, either by means of gum water or by thin paste. The mouth of the jar is furnished with a wooden cover which fits into the inside, and rests on the jar by means of a narrow projecting rim, which forms a shoulder. A stout brass wire passes through the centre of the cover, having a brass chain hanging from its lower extremity to the bottom of the jar, which is also covered with tinfoil, and in contact with the other part of the metallic lining. The upper part of the wire projects two or three inches above the cover, and is surmounted with a brass ball.

If I present the ball of the jar to the ball of the prime conductor, having hold of the coated part of the jar with my hand, a series of sparks are seen to pass between the two balls, from the conductor to the inner surface of the glass jar; but their size and frequency very soon diminish, and eventually they entirely disappear. When this happens, the jar is incapable of receiving any more fluid from the conductor, and is in that condition which we call *charged*. If now, whilst the jar is charged, any person were to touch the outside coating with one hand, and approach the ball on the top of the wire with the other, he would immediately experience a smart blow, or shock, which would affect him more or less accordingly with the size of the jar. This effect is known by the name of *Electric Shock*, and is one method of discharging the jar.

If, instead of one person making the communication between the outside and inside of the jar, a number of persons were to take hold of hands, so as to form a chain from the person at one end in connexion with the outside of the jar, to the person at the other end, who, by his hand, touched the ball at the top, the whole chain of persons would simultaneously receive the shock; but, in this case, the shock experienced by any individual, would be much feebler than if the same person were to discharge the jars himself from one hand to the other. But if a chain of persons were to take hold of a long wire, which reached from the outer coating of a charged jar to the ball at its top, the glass would be more effectually discharged than by the chain of persons alone, and they would be entirely free from the shock, showing that the electric fluid prefers traversing the best conductors in the circuit, which, in this case, was the metallic wire; for, although a portion of the fluid passed through every person in the arrangement, the quantity was so small as scarcely to be productive of any sensible effects. Hence it is, that we can discharge a jar with impunity through a metallic rod, held in the hand, having one of

its extremities in connexion with the metallic coating, and bringing the other extremity close to the brass ball. Any metallic wire employed for that purpose, is called a *discharging rod*.

*Prize Volumes of the Annals of Electricity, &c.*

The Prizes offered for volume seven, will be awarded to the successful competitors on the following subjects:—

1st.—For a description, with drawings, of the most powerful electro-magnet, exceeding a power of two tons. The iron may be of any shape or size, but is to be in one piece, and the armature also in one piece. The power signified is the power of sustaining a load by the force of attraction, whilst the magnet and armature are in close contact with each other. It is expected that the battery employed will be described.

2nd.—For a description of the most powerful *compound* electro-magnet, above a power of two tons. Conditions as in the first.

3rd.—For a voltaic battery of ten pairs, of the greatest decomposing power of acidulated water, with the least cost, including *price of battery*, cost of *exciting liquid*, and *time* necessary to keep it in good action. In short, the most economical battery for decomposing water for one hour continuously, without change of exciting liquid. The metals to expose not less than eighty square inches to the exciting liquid, and to be distributed in any manner the experimenter thinks proper.

4th.—For the best code of theoretical laws for the explanation of the production of *secondary*, *tertiary*, &c. Electric currents, with examples of application.

5th.—For the best set of experiments on the subject of atmospheric electrical waves; with a description of their effects during the exhibition of lightning. The experiments are at the option of the competitors, but are expected to be made with very elevated conductors, either by means of a kite, balloon, tall buildings, or otherwise.

6th.—For the best paper on any novel experimental results in any branch of electricity or magnetism.

The Prize for each of the above subjects will be volume seven of the “Annals of Electricity, Magnetism, Chemistry, &c.,” bound and gold-lettered in the first rate style, with a suitable emblem and motto. To be presented to the successful candidates, or to their agents, (in London, if required,) on the first day of February, 1842. The communications to be made on or before the 10th of December, 1841.

Fig. 2.

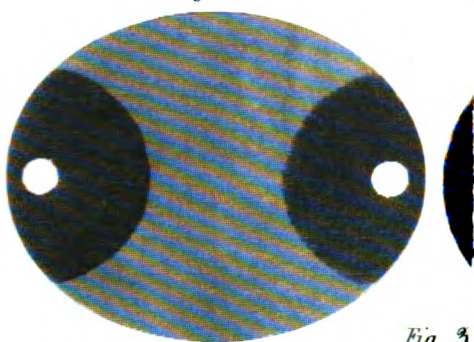


Fig. 3.

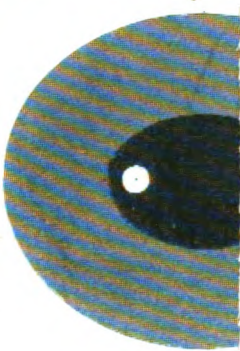
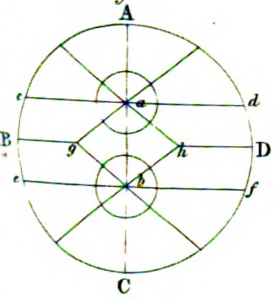
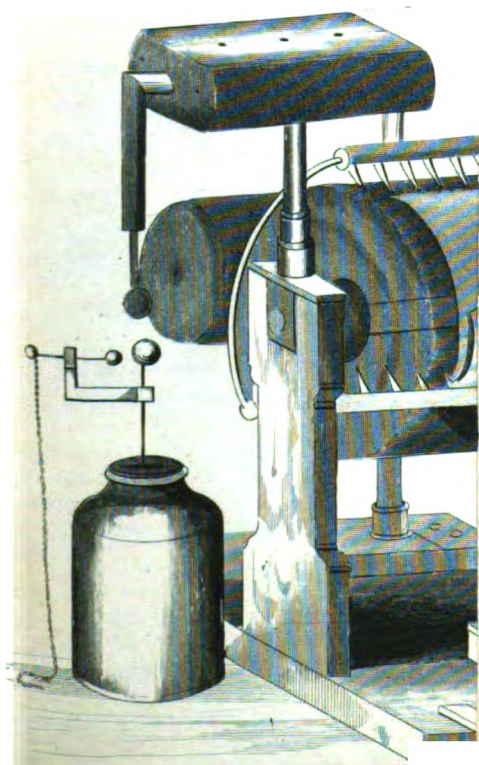


Fig. 4.



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THE ANNALS  
OF  
ELECTRICITY, MAGNETISM,  
AND  
CHEMISTRY;  
*And Guardian of Experimental Science.*

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AUGUST, 1841.

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*Contributions to the Physiology of Vision.—Part the First.*  
*On some remarkable, and hitherto unobserved, Phenomena*  
*of Binocular Vision.* By CHARLES WHEATSTONE, F.R.S.,  
Professor of Experimental Philosophy in King's College,  
London.\*

Received and Read June 21, 1838.

§ 1.

WHEN an object is viewed at so great a distance that the optic axes of both eyes are sensibly parallel when directed towards it, the perspective projections of it, seen by each eye separately, are similar, and the appearance to the two eyes is precisely the same as when the object is seen by one eye only. There is, in such case, no difference between the visual appearance of an object in relief and its perspective projection on a plane surface; and hence pictorial representations of distant objects, when those circumstances which would prevent or disturb the illusion are carefully excluded, may be rendered such perfect resemblances of the objects they are intended to represent as to be mistaken for them; the Diorama is an instance of this. But this similarity no longer exists when the object is placed so near the eyes that to view it the optic axes must converge; under these conditions a different perspective projection of it is seen by each eye, and these perspectives are more dissimilar as the convergence of the optic axes becomes greater. This fact may be easily

\* From Philosophical Transactions of the Royal Society of London.

verified by placing any figure of three dimensions, an outline cube for instance, at a moderate distance before the eyes, and while the head is kept perfectly steady, viewing it with each eye successively while the other is closed. Plate 3. fig 1. represents the two perspective projections of a cube; *b* is that seen by the right eye, and *a* that presented to the left eye; the figure being supposed to be placed about seven inches immediately before the spectator.

The appearances, which are by this simple experiment rendered so obvious, may be easily inferred from the established laws of perspective; for the same object in relief is, when viewed by a different eye, seen from two points of sight at a distance from each other equal to the line joining the two eyes. Yet they seem to have escaped the attention of every philosopher and artist who has treated of the subjects of vision and perspective. I can ascribe this inattention to a phenomenon leading to the important and curious consequences, which will form the subject of the present communication, only to this circumstance; that the results being contrary to a principle which was very generally maintained by optical writers, viz. that objects can be seen single only when their images fall on corresponding points of the two retinæ, an hypothesis which will be hereafter discussed, if the consideration ever arose in their minds, it was hastily discarded under the conviction, that if the pictures presented to the two eyes are under certain circumstances dissimilar, their differences must be so small that they need not be taken into account.

It will now be obvious why it is impossible for the artist to give a faithful representation of any near solid object, that is, to produce a painting which shall not be distinguished in the mind from the object itself. When the painting and the object are seen with both eyes, in the case of the painting two *similar* pictures are projected on the retinæ, in the case of the solid object the pictures are *dissimilar*; there is therefore an essential difference between the impressions on the organs of sensation in the two cases, and consequently between the perceptions formed in the mind; the painting therefore cannot be confounded with the solid object.

After looking over the works of many authors who might be expected to have made some remarks relating to this subject, I have been able to find but one, which is in the *Trattato della Pittura* of LEONARDO DA VINCI.\* This great

\* See also a *Treatise of Painting*, p. 178. London, 1721; and Dr. SMITH's *Complete System of Optics*, vol. ii. r. 244, where the passage is quoted.

artist and ingenious philosopher observes, "that a painting, though conducted with the greatest art and finished to the last perfection, both with regard to its contours, its lights, its shadows and its colours, can never show a relieve equal to that of the natural objects, unless these be viewed at a distance and with a single eye. For," says he, "if an object C (Plate 2. fig. 1.) be viewed by a single eye at A, all objects in the space behind it, included as it were in a shadow E C F cast by a candle at A, are invisible to the eye at A; but when the other eye at B is opened, part of these objects become visible to it, those only being hid from both eyes that are included, as it were, in the double shadow C D, cast by two lights at A and B, and terminated in D, the angular space E D G beyond D being always visible to both eyes. And the hidden space C D is so much the shorter, as the object C is smaller and nearer to the eyes. Thus the object C seen with both eyes becomes, as it were, transparent, according to the usual definition of a transparent thing; namely, that which hides nothing beyond it. But this cannot happen when an object, whose breadth is bigger than that of the pupil, is viewed by a single eye. The truth of this observation is therefore evident, because a painted figure intercepts all the space behind its apparent place, so as to preclude the eyes from the sight of every part of the imaginary ground behind it."

Had LEONARDO DA VINCI taken, instead of a sphere, a less simple figure for the purpose of his illustration, a cube for instance, he would not only have observed that the object obscured from each eye a different part of the more distant field of view, but the fact would also perhaps have forced itself upon his attention, that the object itself presented a different appearance to each eye. He failed to do this, and no subsequent writer within my knowledge has supplied the omission; the projection of two obviously dissimilar pictures on the two retinæ when a single object is viewed, while the optic axes converge, must therefore be regarded as a new fact in the theory of vision.

## § 2.

It being thus established that the mind perceives an object of three dimensions by means of the two dissimilar pictures projected by it on the two retinæ, the following question occurs: What would be the visual effect of simultaneously presenting to each eye, instead of the object itself, its

projection on a plane surface, as it appears to that eye? To pursue this inquiry, it is necessary that means should be contrived to make the two pictures, which must necessarily occupy different places, fall on similar parts of both retinæ. Under the ordinary circumstances of vision the object is seen at the concurrence of the optic axes, and its images consequently are projected on similar parts of the two retinæ; but it is also evident that two exactly similar objects may be made to fall on similar parts of the two retinæ, if they are placed one in the direction of each optic axis, at equal distances before or beyond their intersection.

Fig. 2. represents the usual situation of an object at the intersection of the optic axes. In fig. 3. the similar objects are placed in the direction of the optic axes before their intersection, and in fig. 4. beyond it. In all these three cases the mind perceives but a single object, and refers it to the place where the optic axes meet. It will be observed, that when the eyes converge beyond the objects, as in fig. 3., the right hand object is seen by the right eye, and the left hand object by the left eye; while when the axes converge nearer than the objects, the right hand object is seen by the left eye, and conversely. As both of these modes of vision are forced and unnatural, eyes unaccustomed to such experiments require some artificial assistance. If the eyes are to converge beyond the objects, this may be afforded by a pair of tubes (fig. 5.) capable of being inclined towards each other at various angles, so as to correspond with the different convergences of the optic axes. If the eyes are to converge at a nearer distance than that at which the objects are placed, a box (fig. 6.) may be conveniently employed; the objects *a a'* are placed distant from each other, on a stand capable of being moved nearer the eyes if required, and the optic axes being directed towards them will cross at *c*, the aperture *b b'* allowing the visual rays from the right-hand object to reach the left eye, and those from the left-hand object to fall on the right eye; the coincidence of the images may be facilitated by placing the point of a needle at the point of intersection of the optic axes *c*, and fixing the eyes upon it. In both these instruments (figs. 5. and 6.) the lateral images are hidden from view, and much less difficulty occurs in making the images unite than when the naked eyes are employed.

Now, if instead of placing two exactly similar objects to be viewed by the eyes in either of the modes above described, the two perspective projections of the same solid object be so disposed, the mind will still perceive the object to be single,

but instead of a representation on a plane surface, as each drawing appears to be when separately viewed by that eye which is directed towards it, the observer will perceive a figure of three dimensions, the exact counterpart of the object from which the drawings were made. To make this matter clear, I will mention one or two of the most simple cases.

If two vertical lines near each other, but at different distances from the spectator, be regarded first with one eye, and then with the other, the distance between them when referred to the same plane will appear different; if the left-hand line be nearer to the eyes, the distance as seen by the left eye will be less than the distance as seen by the right eye: fig. 7. will render this evident;  $a a'$  are vertical sections of the two original lines, and  $b b'$  the plane to which their projections are referred. Now if the two lines be drawn on two pieces of card, at the respective distances at which they appear to each eye, and these cards be afterwards viewed by either of the means above directed, the observer will no longer see two lines on a plane surface, as each card separately shows; but two lines will appear, one nearer to him than the other, precisely as the original vertical lines themselves. Again, if a straight wire be held before the eyes in such a position that one of its ends shall be nearer to the observer than the other is, each eye separately referring it to a plane perpendicular to the common axis, will see a line differently inclined; and then if lines having the same apparent inclinations be drawn on two pieces of card, and be presented to the eyes as before directed, the real position of the original line will be correctly perceived by the mind.

In the same manner the most complex figures of three dimensions may be accurately represented to the mind by presenting their two perspective projections to the two retinae. But I shall defer these more perfect experiments until I describe an instrument which will enable any person to observe all the phenomena in question with the greatest ease and certainty.

In the instruments above described the optic axes converge to some point in a plane before or beyond that in which the objects to be seen are situated. The adaptation of the eye, which enables us to see distinctly at different distances, and which habitually accompanies every different degree of convergence of the optic axes, does not immediately adjust itself to the new and unusual condition; and to persons not accustomed to experiments of this kind, the pictures will either not readily unite, or will appear dim and confused. Besides this,

no object can be viewed according to either mode when the drawings exceed in breadth the distance of the two points of the optic axes in which their centres are placed.

These inconveniences are removed by the instrument I am about to describe; the two pictures (or rather their reflected images) are placed in it at the true concourse of the optic axes, the focal adaptation of the eye preserves its usual adjustment, the appearance of lateral images is entirely avoided, and a large field of view for each eye is obtained. The frequent reference I shall have occasion to make to this instrument, will render it convenient to give it a specific name, I therefore propose that it be called a Stereoscope, to indicate its property of representing solid figures.

### § 3.

The stereoscope is represented by figs. 8. and 9; the former being a front view, and the latter a plan of the instrument. *A A'* are two plane mirrors, about four inches square, inserted in frames, and so adjusted that their backs form an angle of  $90^\circ$  with each other; these mirrors are fixed by their common edge against an upright *B*, or, which was less easy to represent in the drawing, against the middle line of a vertical board, cut away in such a manner as to allow the eyes to be placed before the two mirrors. *C C'* are two sliding boards, to which are attached the upright boards *D D'*, which may thus be removed to different distances from the mirrors. In most of the experiments hereafter to be detailed, it is necessary that each upright board shall be at the same distance from the mirror which is opposite to it. To facilitate this double adjustment, I employ a right and a left-handed wooden screw, *r l*; the two ends of this compound screw pass through the nuts *e e'*, which are fixed to the lower parts of the upright boards *D D'*, so that by turning the screw pin *p* one way the two boards will approach, and by turning it the other they will recede from each other, one always preserving the same distance as the other from the middle of the line *f*. *E E'* are pannels, to which the pictures are fixed in such manner that their corresponding horizontal lines shall be on the same level: these pannels are capable of sliding backwards and forwards in grooves on the upright boards *D D'*. The apparatus having been described, it now remains to explain the manner of using it. The observer must place his eyes as near as possible to the mirrors, the right eye before the right-hand mirror, and the left eye before the left-hand

mirror, and he must move the sliding pannels *E E'* to or from him until the two reflected images coincide at the intersection of the optic axes, and form an image of the same apparent magnitude as each of the component pictures. The pictures will indeed coincide when the sliding pannels are in a variety of different positions, and consequently when viewed under different inclinations of the optic axes; but there is only one position in which the binocular image will be immediately seen single, of its proper magnitude, and without fatigue to the eyes, because in this position only the ordinary relations between the magnitude of the pictures on the retina, the inclination of the optic axes, and the adaptation of the eye to distinct vision at different distances are preserved. The alteration in the apparent magnitude of the binocular images, when these usual relations are disturbed, will be discussed in another paper of this series, with a variety of remarkable phenomena depending thereon. In all the experiments detailed in the present memoir I shall suppose these relations to remain undisturbed, and the optic axes to converge about six or eight inches before the eyes.

If the pictures are all drawn to be seen with the same inclination of the optic axes, the apparatus may be simplified by omitting the screw *r l* and fixing the upright boards *D D'* at the proper distances. The sliding pannels may also be dispensed with, and the drawings themselves be made to slide in the grooves.

#### § 4.

A few pairs of outline figures, calculated to give rise to the perception of objects of three dimensions when placed in the stereoscope in the manner described, are represented from figs. 10. to 20. They are one half the linear size of the figures actually employed. As the drawings are reversed by reflection in the mirrors, I will suppose these figures to be the reflected images to which the eyes are directed in the apparatus; those marked *b* being seen by the right eye, and those marked *a* by the left eye. The drawings, it has been already explained, are two different projections of the same object seen from two points of sight, the distance between which is equal to the interval between the eyes of the observer; this interval is generally about  $2\frac{1}{2}$  inches.

*a* and *b*, fig. 10. will, when viewed in the stereoscope, present to the mind a line in the vertical plane, with its lower end inclined towards the observer. If the two

component lines be caused to turn round their centres equally in opposite directions, the resultant line will, while it appears to assume every degree of inclination to the referent plane, still seem to remain in the same vertical plane.

Fig. 11. A series of points all in the same horizontal plane, but each towards the right hand successively nearer the observer.

Fig. 12. A curved line intersecting the referent plane, and having its convexity towards the observer.

Fig. 13. A cube.

Fig. 14. A cone, having its axis perpendicular to the referent plane, and its vertex towards the observer.

Fig. 15. The frustrum of a square pyramid; its axis perpendicular to the referent plane, and its base furthest from the eye.

Fig. 16. Two circles at different distances from the eyes, their centres in the same perpendicular, forming the outline of the frustrum of a cone.

The other figures require no observation.

For the purposes of illustration I have employed only outline figures, for had either shading or colouring been introduced, it might be supposed that the effect was wholly or in part due to these circumstances, whereas, by leaving them out of consideration, no room is left to doubt that the entire effect of relief is owing to the simultaneous perception of the two monocular projections, one on each retina. But if it be required to obtain the most faithful resemblances of real objects, shadowing and colouring may properly be employed to heighten the effects. Careful attention would enable an artist to draw and paint the two component pictures, so as to present to the mind of the observer, in the resultant perception, perfect identity with the object represented. Flowers, crystals, busts, vases, instruments of various kinds, &c., might thus be represented so as not to be distinguished by sight from the real objects themselves.

It is worthy of remark, that the process by which we thus become acquainted with the real forms of solid objects, is precisely that which is employed in descriptive geometry, an important science we owe to the genius of MONGE, but which is little studied or known in this country. In this science, the position of a point, a right line, or a curve, and consequently of any figure whatever, is completely determined by assigning its projections on two fixed planes, the situations of which are known, and which are not parallel to



each other. In the problems of descriptive geometry the two referent planes are generally assumed to be at right angles to each other, but in binocular vision the inclination of these planes is less according as the angle made at the concourse of the optic axes is less; thus the same solid object is represented to the mind by different pairs of monocular pictures, according as they are placed at a different distance before the eyes, and the perception of these differences (though we seem to be unconscious of them) may assist in suggesting to the mind the distance of the object. The more inclined to each other the referent planes are, with the greater accuracy are the various points of the projections referred to their proper places; and it appears to be a useful provision that the real forms of those objects which are nearer to us are thus more determinately apprehended than those which are more distant.

### § 5.

A very singular effect is produced when the drawing originally intended to be seen by the right eye is placed at the left hand side of the stereoscope, and that designed to be seen by the left eye is placed on its right hand side. A figure of three dimensions, as bold in relief as before, is perceived, but it has a different form from that which is seen when the drawings are in their proper places. There is a certain relation between the proper figure and this, which I shall call its *converse* figure. Those points which are nearest the observer in the proper figure are the most remote from him in the converse figure, and *vice versâ*, so that the figure is, as it were, inverted; but it is not an exact inversion, for the near parts of the converse figure appear smaller, and the remote parts larger than the same parts before the inversion. Hence the drawings which, properly placed, occasion a cube to be perceived, when changed in the manner described, represent the frustum of a square pyramid with its base remote from the eye: the cause of this is easy to understand.

This conversion of relief may be shown by all the pairs of drawings from fig. 10 to 19. In the case of simple figures like these the converse figure is as readily apprehended as the original one, because it is generally a figure of as frequent occurrence; but in the case of a more complicated figure, an architectural design, for instance, the mind, unaccustomed to perceive its converse, because it never occurs in nature, can find no meaning in it.

## § 6.

The same image is depicted on the retina by an object of three dimensions as by its projection on a plane surface, provided the point of sight remain in both cases the same. There should be, therefore, no difference in the binocular appearance of two drawings, one presented to each eye, and of two real objects so presented to the two eyes that their projections on the retina shall be the same as those arising from the drawings. The following experiments will prove the justness of this inference.

I procured several pairs of skeleton figures, i. e. outline figures of three dimensions, formed either of iron wire or of ebony beading about one tenth of an inch in thickness. The pair I most frequently employed consisted of two cubes, whose sides were three inches in length. When I placed these skeleton figures on stands before the two mirrors of the stereoscope, the following effects were produced, according as their relative positions were changed. 1st. When they were so placed that the pictures which their reflected images projected on the two retinæ were precisely the same as those which would have been projected by a cube placed at the concurrence of the optic axes, a cube in relief appeared before the eyes. 2ndly. When they were so placed that their reflected images projected exactly similar pictures on the two retinæ, all effect of relief was destroyed, and the compound appearance was that of an outline representation on a plane surface. 3rdly. When the cubes were so placed that the reflected image of one projected on the left retina the same picture as in the first case was projected on the right retina, and conversely, the converse figure in relief appeared.

## § 7.

If a symmetrical object, that is one whose right and left sides are exactly similar to each other but inverted, be placed so that any point in the plane which divides it into these two halves is equally distant from the two eyes, its two monocular projections are, it is easy to see, inverted fac-similes of each other. Thus fig. 15, *a* and *b* are symmetrical monocular projections of the frustum of a four-sided pyramid, and figs. 13. 14. 16. are corresponding projections of other symmetrical objects. This being kept in view, I will describe an experiment which, had it been casually observed previous to the

knowledge of the principles developed in this paper, would have appeared an inexplicable optical illusion.

M and M' (fig. 21.) are two mirrors, inclined so that their *faces* form an angle of  $90^\circ$  with each other. Between them in the bisecting plane is placed a plane outline figure, such as fig. 15 *a*, made of card all parts but the lines being cut away, or of wire. A reflected image of this outline, placed at A, will appear behind each mirror at B and B', and one of these images will be the inversion of the other. If the eyes be made to converge at C, it is obvious that these two reflected images will fall on corresponding parts of the two retinae, and a figure of three dimensions will be perceived; if the outline placed in the bisecting plane be reversed, the converse skeleton form will appear; in both these experiments we have the singular phenomenon of the conversion of a single plane outline into a figure of three dimensions. To render the binocular object more distinct, concave lenses may be applied to the eyes; and to prevent the two lateral images from being seen, screens may be placed at D and D'.

### § 8.

An effect of binocular perspective may be remarked in a plate of metal, the surface of which has been made smooth by turning it in a lathe. When a single candle is brought near such a plate, a line of light appears standing out from it, one half being above, and the other half below the surface; the position and inclination of this line changes with the situation of the light and of the observer, but it always passes through the centre of the plate. On closing the left eye the relief disappears, and the luminous line coincides with one of the diameters of the plate; on closing the right eye the line appears equally in the plane of the surface, but coincides with another diameter; on opening both eyes it instantly starts into relief.\* The case here is exactly analogous to the vision of two inclined lines (fig. 10.) when each is presented to a different eye in the stereoscope. It is curious, that an effect like this, which must have been seen thousands of times, should never have attracted sufficient attention to have been made the subject of philosophic observation. It was one of the earliest facts which drew my attention to the subject I am now treating.

\* The luminous line seen by a single eye arises from the reflection of the light from each of the concentric circles produced in the operation of turning; when the plate is not large the arrangement of these successive reflections does not differ from a straight line.

Dr. SMITH\* was very much puzzled by an effect of binocular perspective which he observed, but was unable to explain. He opened a pair of compasses, and while he held the joint in his hand, and the points outwards and equidistant from his eyes, and somewhat higher than the joint, he looked at a more distant point; the compasses appeared double. He then compressed the legs until the two inner points coincided; having done this the two inner legs also entirely coincided, and bisected the angle formed by the outward ones, appearing longer and thicker than they did, and reaching from the hand to the remotest object in view. The explanation offered by Dr. SMITH accounts only for the coincidence of the points of the compasses, not for that of the entire leg. The effect in question is best seen by employing a pair of straight wires, about a foot in length. A similar observation, made with two flat rulers, and afterwards with silk threads, induced Dr. WELLS to propose a new theory of visible direction in order to explain it, so inexplicable did it seem to him by any of the received theories.

## § 9.

The preceding experiments render it evident that there is an essential difference in the appearance of objects when seen with two eyes, and when only one eye is employed, and that the most vivid belief of the solidity of an object of three dimensions arises from two different perspective projections of it being simultaneously presented to the mind. How happens it then, it may be asked, that persons who see with only one eye form correct notions of solid objects, and never mistake them for pictures? and how happens it also, that a person having the perfect use of both eyes, perceives no difference in objects around him when he shuts one of them? To explain these apparent difficulties, it must be kept in mind, that although the simultaneous vision of two dissimilar pictures suggests the relief of objects in the most vivid manner, yet there are other signs which suggest the same ideas to the mind, which, though more ambiguous than the former, become less liable to lead the judgment astray in proportion to the extent of our previous experience. The vividness of relief arising from the projection of two dissimilar pictures, one on each retina, becomes less and less as the object is seen at a greater distance before the eyes, and entirely ceases when

\* System of Optics, vol. ii. p. 388. and r. 526.

it is so distant that the optic axes are parallel while regarding it. We see with both eyes all objects beyond this distance precisely as we see near objects with a single eye; for the pictures on the two retinae are then exactly similar, and the mind appreciates no difference whether two identical pictures fall on corresponding parts of the two retinae, or whether one eye is impressed with only one of these pictures. A person deprived of the sight of one eye sees therefore all external objects, near and remote, as a person with both eyes sees remote objects only, but that vivid effect arising from the binocular vision of near objects is not perceived by the former; to supply this deficiency he has recourse unconsciously to other means of acquiring more accurate information. The motion of the head is the principal means he employs. That the required knowledge may be thus obtained will be evident from the following considerations. The mind associates with the idea of a solid object every different projection of it which experience has hitherto afforded; a single projection may be ambiguous, from its being also one of the projections of a picture, or of a different solid object; but when different projections of the same object are successively presented, they cannot all belong to another object, and the form to which they belong is completely characterized. While the object remains fixed, at every movement of the head it is viewed from a different point of sight, and the picture on the retina consequently continually changes.

Every one must be aware how greatly the perspective effect of a picture is enhanced by looking at it with only one eye, especially when a tube is employed to exclude the vision of adjacent objects, whose presence might disturb the illusion. Seen under such circumstances from the proper point of sight, the picture projects the same lines, shades and colours on the retina, as the more distant scene which it represents would do were it substituted for it. The appearance which would make us certain that it is a picture is excluded from the sight, and the imagination has room to be active. Several of the older writers erroneously attributed this apparent superiority of monocular vision to the concentration of the visual power in a single eye.\*

There is a well-known and very striking illusion of perspective which deserves a passing remark, because the reason

\* "We see more exquisitely with one eye shut than with both, because the vital spirits thus unite themselves the more, and become the stronger: for we may find by looking in a glass whilst we shut one eye, that the pupil of the other dilates."—Lord Bacon's Works, *Sylva Sylvarum*, art. Vision.

of the effect does not appear to be generally understood. When a perspective of a building is projected on a horizontal plane, so that the point of sight is in a line greatly inclined towards the plane, the building appears to a single eye placed at the point of sight to be in bold relief, and the illusion is almost as perfect as in the binocular experiments described in Sections 2, 3, 4. This effect wholly arises from the unusual projection, which suggests to the mind more readily the object itself than the drawing of it; for we are accustomed to see real objects in almost every point of view, but perspective representations being generally made in a vertical plane with the point of sight in a line perpendicular to the plane of projection, we are less familiar with the appearance of other projections. Any other unusual projection will produce the same effect.

## § 10.

If we look with a single eye at the drawing of a solid geometrical figure, it may be imagined to be the representation of either of two dissimilar solid figures, the figure intended to be represented, or its converse figure (Section 5.) If the former is a very usual, and the latter a very unusual figure, the imagination will fix itself on the original without wandering to the converse figure; but if both are of ordinary occurrence, which is generally the case with regard to simple forms, a singular phenomenon takes place; it is perceived at one time distinctly as one of these figures, at another time as the other, and while one figure continues it is not in the power of the will to change it immediately.

The same phenomenon takes place, though less decidedly, when the drawing is seen with both eyes. Many of my readers will call to mind the puzzling effect of some of the diagrams annexed to the problems of the eleventh book of Euclid; which, when they were attentively looked at, changed in an arbitrary manner from one solid figure to another, and would obstinately continue to present the converse figures when the real figures alone were wanted. This perplexing illusion must be of common occurrence, but I have only found one recorded observation relating to the subject. It is by Professor NÉCKER of Geneva, and I shall quote it in his own words from the *Philosophical Magazine*, Third Series, vol. i. p. 337.

“The object I have now to call your attention to is an observation which has often occurred to me while examining figures and engraved plates of crystalline forms; I mean a

sudden and involuntary change in the apparent position of a crystal or solid represented in an engraved figure. What I mean will be more easily understood from the figure annexed (fig. 22.) The rhomboid A X is drawn so that the solid angle A should be seen the nearest to the spectator, and the solid angle X the farthest from him, and the face A C D B should be the foremost, while the face X D C is behind. But in looking repeatedly at the same figure, you will perceive that at times the apparent position of the rhomboid is so changed that the solid angle X will appear the nearest, and the solid angle A the farthest; and that the face A C D B will recede behind the face X D C, which will come forward, which effect gives to the whole solid a quite contrary apparent inclination."

Professor NECKER attributes this alteration of appearance, not to a mental operation, but to an involuntary change in the adjustment of the eye for obtaining distinct vision. He supposed that whenever the point of distinct vision on the retina is directed on the angle A, for instance, this angle seen more distinctly than the others is naturally supposed to be nearer and foremost, while the other angles seen indistinctly are supposed to be farther and behind, and that the reverse takes place when the point of distinct vision is brought to bear on the angle X.

That this is not the true explanation, is evident from three circumstances: in the first place, the two points A and X being both at the same distance from the eyes, the same alteration of adjustment which would make one of them indistinct would make the other so; secondly, the figure will undergo the same changes whether the focal distance of the eye be adjusted to a point before or beyond the plane in which the figure is drawn; and thirdly, the change of figure frequently occurs while the eye continues to look at the same angle. The effect seems entirely to depend on our mental contemplation of the figure intended to be represented, or of its converse. By following the lines with the eye with a clear idea of the solid figure we are describing, it may be fixed for any length of time; but it requires practice to do this or to change the figure at will. As I have before observed, these effects are far more obvious when the figures are regarded with one eye only.

No illusion of this kind can take place when an object of three dimensions is seen with both eyes while the optic axes make a sensible angle with each other, because the appearance of the two dissimilar images, one to each eye, prevents

the possibility of mistake. But if we regard an object at such a distance that its two projections are sensibly identical, and if this projection be capable of a double interpretation, the illusion may occur. Thus a placard on a pole carried in the streets, with one of its sides inclined towards the observer, will, when he is distant from it, frequently appear inclined in a contrary direction. Many analogous instances might be adduced, but this will suffice to call others to mind; it must however be observed, that when shadows, or other means capable of determining the judgment are present, these fallacies do not arise.

## § 11.

The same indetermination of judgment which causes a drawing to be perceived by the mind at different times as two different figures, frequently gives rise to a false perception when objects in relief are regarded with a single eye. The apparent conversion of a cameo into an intaglio, and of an intaglio into a cameo, is a well-known instance of this fallacy in vision; but the fact does not appear to me to have been correctly explained, nor the conditions under which it occurs to have been properly stated.

This curious illusion, which has been the subject of much attention, was first observed at one of the early meetings of the Royal Society.\* Several of the members looking through a compound microscope of a new construction, at a guinea, some of them imagined the image to be depressed, while others thought it to be embossed, as it really was. Professor GMELIN, of Wurtemberg, published a paper on the same subject in the *Philosophical Transactions* of 1745; his experiments were made with telescopes, and compound microscopes, which inverted the images; and he observed that the conversion of relief appeared in some cases and not in others, at some times and not at others, and to some eyes also and not to others. He endeavoured to ascertain some of the conditions of the two appearances; "but why these things should so happen," says he, "I do not pretend to determine."

Sir DAVID BREWSTER accounts for the fallacy in the following manner†:—"A hollow seal being illuminated by a window or a candle, its shaded side is of course on the same side with the light. If we now invert the seal with one or more lenses, so that it may look in the opposite direction, it

\* BIRCH's *History*, vol. ii. p. 348.† *Natural Magic*, p. 100.



will appear to the eye with the shaded side furthest from the window. But as we know that the window is still on our left hand, and as every body with its shaded side furthest from the light must necessarily be convex or protuberant, we immediately believe that the hollow seal is now a cameo or bas-relief. The proof which the eye thus receives of the seal being raised, overcomes the evidence of its being hollow, derived from our actual knowledge, and from the sense of touch. In this experiment the deception takes place from our knowing the real direction of the light which falls on the seal; for if the place of the window, with respect to the seal, had been inverted as well as the seal itself, the illusion could not have taken place. The illusion, therefore, under our consideration is the result of an operation of our own minds, whereby we judge of the forms of bodies by the knowledge we have acquired of light and shadow. Hence the illusion depends on the accuracy and extent of our knowledge on this subject; and while some persons are under its influence, others are entirely insensible to it."

These considerations do not fully explain the phenomenon, for they suppose that the image must be inverted, and that the light must fall in a particular direction; but the conversion of relief will still take place when the object is viewed through an open tube, without any lenses to invert it, and also when it is equally illuminated in all parts. The true explanation I believe to be the following. If we suppose a cameo and an intaglio of the same object, the elevations of the one corresponding exactly to the depressions of the other, it is easy to show that the projections of either on the retina is sensibly the same. When the cameo or intaglio is seen with both eyes, it is impossible to mistake an elevation for a depression, for reasons which have been already amply explained; but when either is seen with one eye only, the most certain guide of our judgment, viz. the presentation of a different picture to each eye, is wanting; the imagination therefore supplies the deficiency, and we conceive the object to be raised or depressed according to the dictates of this faculty. No doubt in such cases our judgment is in a great degree influenced by accessory circumstances, and the intaglio or the relief may sometimes present itself according to our previous knowledge of the direction in which the shadows ought to appear; but the real cause of the phenomenon is to be found in the indetermination of the judgment arising from our more perfect means of judging being absent.

Observers with the microscope must be particularly on

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their guard against illusions of this kind. RASPAIL observes\* that the hollow pyramidal arrangement of the crystals of muriate of soda appears, when seen through a microscope, like a striated pyramid in relief. He recommends two modes of correcting the illusion. The first is to bring successively to the focus of the instrument the different parts of the crystal; if the pyramid be in relief, the point will arrive at the focus sooner than the base will; if the pyramid be hollow the contrary will take place. The second mode is to project a strong light on the pyramid in the field of view of the microscope, and to observe which sides of the crystal are illuminated, taking however the inversion of the image into consideration if a compound microscope be employed.

The inversion of relief is very striking when a skeleton cube is looked at with one eye, and the following singular results may in this case be observed. So long as the mind perceives the cube, however the figure be turned about, its various appearances will be but different representations of the same object, and the same primitive form will be suggested to the mind by all of them: but it is not so if the converse figure fixes the attention; the series of successive projections cannot then be referred to any figure to which they are all common, and the skeleton figure will appear to be continually undergoing a change of shape.

## § 12.

I have given ample proof that objects whose pictures do not fall on corresponding points of the two retinæ may still appear single. I will now adduce an experiment which proves that similar pictures falling on corresponding points of the two retinæ may appear double and in different places.

Present, in the stereoscope, to the right eye a vertical line, and to the left eye a line inclined some degrees from the perpendicular (fig. 23.); the observer will then perceive, as formerly explained, a line, the extremities of which appear at different distances before the eyes. Draw on the left hand figure a faint vertical line exactly corresponding in position and length to that presented to the right eye, and let the two lines of this left hand figure intersect each other at their centres. Looking now at these two drawings in the stereoscope, the two strong lines, each seen by a different eye, will coincide, and the resultant perspective line will appear to

\* Nouveau Système de Chimie Organique, 2<sup>me</sup> edit. t. 1. p. 333.

occupy the same place as before; but the faint line which now falls on a line of the left retina, which corresponds with the line of the right retina on which one of the coinciding strong lines, viz. the vertical one, falls, appears in a different place. The place this faint line apparently occupies is the intersection of that plane of visual direction of the left eye in which it is situated, with the plane of visual direction of the right eye, which contains the strong vertical line.

This experiment affords another proof that there is no necessary physiological connection between the corresponding points of the two retinæ,—a doctrine which has been maintained by so many authors.

### § 13. *Binocular Vision of Images of different Magnitudes.*

We will now inquire what effect results from presenting similar images, differing only in magnitude, to analogous parts of the two retinæ. For this purpose two squares or circles, differing obviously but not extravagantly in size, may be drawn on two separate pieces of paper, and placed in the stereoscope so that the reflected image of each shall be equally distant from the eye by which it is regarded. It will then be seen that, notwithstanding this difference, they coalesce and occasion a single resultant perception. The limit of the difference of size within which the single appearance subsists may be ascertained by employing two images of equal magnitude, and causing one of them to recede from the eye while the other remains at a constant distance; this is effected merely by pulling out the sliding board C (fig. 8.) while the other C' remains fixed, the screw having previously been removed.

Though the single appearance of two images of different size is by this experiment demonstrated, the observer is unable to perceive what difference exists between the apparent magnitude of the binocular image and that of the two monocular images; to determine this point the stereoscope must be dispensed with, and the experiment so arranged that all three shall be simultaneously seen; which may be done in the following manner:—The two drawings being placed side by side on a plane before the eyes, the optic axes must be made to converge to a nearer point as at fig. 4., or to a more distant one as at fig. 3., until the three images are seen at the same time, the binocular image in the middle, and the monocular images at each side. It will thus be seen that the binocular image is apparently intermediate in size between the two monocular ones.

If the pictures be too unequal in magnitude, the binocular coincidence does not take place. It appears that if the inequality of the pictures be greater than the difference which exists between the two projections of the same object when seen in the most oblique position of the eyes (*i. e.* both turned to the extreme right or to the extreme left), ordinarily employed, they do not coalesce. Were it not for the binocular coincidence of two images of different magnitude, objects would appear single only when the optic axes converge immediately forwards; for it is only when the converging visual lines form equal angles with the visual base (the line joining the centres of the two eyes) as at fig. 2., that the two pictures can be of equal magnitude; but when they form different angles with it, as at fig. 24., the distance from the object to each eye is different, and consequently the picture projected on each retina has a different magnitude. If a piece of money be held in the position *a*, (fig. 24.) while the optic axes converge to a nearer point *c*, it will appear double, and that seen by the left eye will be evidently smaller than the other.

§ 14. *Phenomena which are observed when objects of different forms are simultaneously presented to corresponding parts of the two retinæ.*

If we regard a picture with the right eye alone for a considerable length of time it will be constantly perceived; if we look at another and dissimilar picture with the left eye alone its effect will be equally permanent; it might therefore be expected, that if each of these pictures were presented to its corresponding eye at the same time the two would appear permanently superposed on each other. This, however, contrary to expectation, is not the case.

If *a* and *b* (fig. 25.) are each presented at the same time to a different eye, the common border will remain constant, while the letter within it will change alternately from that which would be perceived by the right eye alone to that which would be perceived by the left eye alone. At the moment of change the letter which has just been seen breaks into fragments, while fragments of the letter which is about to appear mingle with them, and are immediately after replaced by the entire letter. It does not appear to be in the power of the will to determine the appearance of either of the letters, but the duration of the appearance seems to depend on causes which are under our control: thus if the

two pictures be equally illuminated, the alternations appear in general of equal duration; but if one picture be more illuminated than the other, that which is less so will be perceived during a shorter time. I have generally made this experiment with the apparatus fig. 6. When complex pictures are employed in the stereoscope, various parts of them alternate differently.

There are some facts intimately connected with the subject of the present article which have already been frequently observed. I allude to the experiments, first made by Du TOUR, in which two different colours are presented to corresponding parts of the two retinae. If a blue disc be presented to the right eye and a yellow disc to the corresponding part of the left eye, instead of a green disc which would appear if these two colours had mingled before their arrival at a single eye, the mind will perceive the two colours distinctly one or the other alternately predominating either partially or wholly over the disc. In the same manner the mind perceives no trace of violet when red is presented to one eye and blue to the other, nor any vestige of orange when red and yellow are separately presented in a similar manner. These experiments may be conveniently repeated by placing the coloured discs in the stereoscope, but they have been most usually made by looking at a white object through differently coloured glasses, one applied to each eye.

In some authors we find it stated, contrary to fact, that if similar objects of different colour be presented one to each eye, the appearance will be that compounded of the two colours. Dr. REID\* and JANIN are among the writers who have fallen into this inconsiderate error, which arose no doubt from their deciding according to previous notions, instead of ascertaining by experiment what actually does happen.

### § 15.

No question relating to vision has been so much debated as the cause of the single appearance of objects seen by both eyes. I shall in the present section give a slight review of the various theories which have been advanced by philosophers to account for this phenomenon, in order that the remarks I have to make in the succeeding section may be properly understood.

The law of visible direction for monocular vision has been

\* Enquiry, Sect. xiii.

variously stated by different optical writers. Some have maintained with Drs. REID and PORTERFIELD, that every external point is seen in the direction of a line passing from its picture on the retina through the centre of the eye; while others have supposed with Dr. SMITH that the visible direction of an object coincides with the visual ray, or the principal ray of the pencil which flows from it to the eye. D'ALEMBERT, furnished with imperfect data respecting the refractive densities of the humours of the eye, calculated that the apparent magnitudes of objects would differ widely on the two suppositions, and concluded that the visible point of an object was not seen in either of these directions, but sensibly in the direction of a line joining the point itself and its image on the retina; but he acknowledged that he could assign no reason for this law. Sir DAVID BREWSTER, provided with more accurate data, has shown that these three lines so nearly coincide with each other, that "at an inclination of  $30^\circ$  a line perpendicular to the point of impression on the retina passes through the common centre, and does not deviate from the real line of visible direction more than half a degree, a quantity too small to interfere with the purposes of vision." We may, therefore, assume in all our future reasonings, the truth of the following definition given by this eminent philosopher:—"As the interior eye ball is as nearly as possible a perfect sphere, lines perpendicular to the surface of the retina must all pass through one single point, namely the centre of its spherical surface. This one point may be called the centre of visible direction, because every point of a visible object will be seen in the direction of a line drawn from this centre to the visible point."

It is obvious that the result of any attempt to explain the single appearance of objects to both eyes, or in other words, the law of visible direction for binocular vision, ought to contain nothing inconsistent with the law of visible direction for monocular vision.

It was the opinion of AGUILONIUS, that all objects seen at the same glance with both eyes appear to be in the plane of the horopter. The horopter he defines to be a line drawn through the point of intersection of the optic axes, and parallel to the line joining the centres of the two eyes; the plane of the horopter to be a plane passing through this line at right angles to that of the optic axes. All objects which are in this plane, must, according to him, appear single because the lines of direction in which any point of an object is seen coincide only in this plane and nowhere else; and as

these lines can meet each other only in one point, it follows from the hypothesis, that all objects not in the plane of the horopter must appear double, because their lines of direction intersect each other, either before or after they pass through it. This opinion was also maintained by DECHALES and PORTERFIELD. That it is erroneous, I have given, I think, sufficient proof, in showing that, when the optic axes converge to any point, objects before or beyond the plane of the horopter are under certain circumstances equally seen single as those in that plane.

Dr. WELLS's "new theory of visible direction" was a modification of the preceding hypothesis. This acute writer held with AGUILONIUS, that objects are seen single only when they are in the plane of the horopter, and consequently that they appear double when they are either before or beyond it; but he attempted to make this single appearance of objects only in the plane of the horopter to depend on other principles, from which he deduced, contrary to AGUILONIUS, that the objects which are doubled do not appear in the plane of the horopter, but in other places which are determined by these principles. Dr. WELLS was led to his new theory by a fact which he accidentally observed, and which he could not reconcile with any existing theory of visible direction: this fact had, though he was unaware of it, been previously noticed by Dr. SMITH; it is already mentioned in § 8., and is the only instance of binocular vision of relief which I have found recorded previous to my own investigations. So little does Dr. WELLS's theory appear to have been understood, that no subsequent writer has attempted either to confirm or disprove his opinions. It would be useless here to discuss the principles of this theory, which was framed to account for an anomalous individual fact, since it is inconsistent with the general rules on which that fact has been now shown to depend. Notwithstanding these erroneous views, the "essay upon single vision with two eyes" contains many valuable experiments and remarks, the truth of which are independent of the theory they were intended to illustrate.

The theory which has obtained greatest currency is that which assumes that an object is seen single because its pictures fall on corresponding points of the two retinae, that is, on points which are similarly situated with respect to the two centres both in distance and position. This theory supposes that the pictures projected on the retinae are exactly similar to each other, corresponding points of the two pictures falling

on corresponding points of the two retinae. Authors who agree with regard to this property, differ widely in explaining why objects are seen in the same place, or single, according to this law. Dr. SMITH makes it to depend entirely on custom, and explains why the eyes are habitually directed towards an object so that its pictures fall on corresponding parts in the following manner:—"When we view an object steadily, we have acquired a habit of directing the optic axes to the point in view; because its pictures falling upon the middle points of the retinas, are then distincter than if they fell upon any other places; and since the pictures of the whole object are equal to one another, and are both inverted with respect to the optic axes, it follows that the pictures of any collateral point are painted upon corresponding points of the retinas."

Dr. REID, after a long dissertation on the subject, concludes, "that by an original property of human eyes, objects painted upon the centres of the two retinae, or upon points similarly situated with regard to the centres, appear in the same visible place; that the most plausible attempts to account for this property of the eyes have been unsuccessful; and therefore, that it must be either a primary law of our constitution, or the consequence of some more general law which is not yet discovered."

Other writers who have admitted this principle have regarded it as arising from anatomical structure and dependent on connexion of nervous fibres; among these stand the names of GALEN, Dr. BRIGGS, Sir ISAAC NEWTON, ROHAULT, Dr. HARTLEY, Dr. WOOLLASTON and Professor MÜLLER.

Many of the supporters of the theory of corresponding points, have thought, or rather have admitted, *without thinking*, that it was not inconsistent with the law of AGUILONIUS; but very little reflection will show that both cannot be maintained together; for corresponding lines of visible direction, that is, lines terminating in corresponding points of the two retinae, cannot meet in the plane of the horopter unless the optic axes be parallel, and the plane be at an infinite distance before the eyes. Some of the modern German writers\* have inquired what is the curve in which objects appear single while the optic axes are directed to a given point, on the hypothesis that objects are seen single only when they fall on corresponding points of the two retinae.

\* *Tortual, die Sinne des Menschen*. Münster, 1827. *Bartels, Beiträge zur Physiologie der Gesichtssinnes*. Berlin, 1834.



An elegant proposition has resulted from their investigations, which I shall need no apology for introducing in this place, since it has not yet been mentioned in any English work.

R and L (fig. 26.) are the two eyes; C A, C' A the optic axes converging to the point A; and C A B C' is a circle drawn through the point of convergence A and the centres of visible direction C C'. If any point be taken in the circumference of this circle, and lines be drawn from it through the centres of the two eyes C C', these lines will fall on corresponding points of the two retinae D D'; for the angles A C B, A C' B being equal, the angles D C E, D C' E are also equal; therefore, any point placed in the circumference of the circle C A B C' will, according to the hypothesis, appear single while the optic axes are directed to A, or any other part in it.

I will mention two other properties of this binocular circle: 1st. The arc subtended by two points on its circumference contains double the number of degrees of the arc subtended by the pictures of these points on either retina, so that objects which occupy  $180^\circ$  of the supposed circle of single vision are painted on a portion of the retina extended over  $9^\circ$  only; for the angle D C E or D C' E being at the centre, and the angle B C A or B C' A at the circumference of a circle, this consequence follows. 2ndly. To whatever point of the circumference of the circle the optic axes be made to converge, they will form the same angle with each other; for the angles C A C', C B C are equal.

In the eye itself, the centre of visible direction, or the point at which the principal rays cross each other, is, according to Dr. YOUNG and other eminent optical writers, at the same time the centre of the spherical surface of the retina, and that of the lesser spherical surface of the cornea; in the diagram (fig. 26.), to simplify the consideration of the problem, R and L represent only the circle of curvature of the bottom of the retina, but the reasoning is equally true in both cases.

The same reasons, founded on the experiments in this memoir, which disprove the theory of AGUILONIUS, induce me to reject the law of corresponding points as an accurate expression of the phenomena of single vision. According to the former, objects can appear single only in the plane of the horopter; according to the latter, only when they are in the circle of single vision; both positions are inconsistent with the binocular vision of objects in relief, the points of which they consist appearing single though they are at different distances before the eyes. I have already proved that the

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assumption made by all the maintainers of the theory of corresponding points, namely that the two pictures projected by any object in the retinae are exactly similar, is quite contrary to fact in every case except that in which the optic axes are parallel.

GASSENDUS, PORTA, TACQUET and GALL maintained, that we see with only one eye at a time though both remain open, one according to them being relaxed and inattentive to objects while the other is upon the stretch. It is a sufficient refutation of this hypothesis, that we see an object double when one of the optic axes is displaced either by squinting or by pressure on the eye-ball with the finger; if we saw with only one eye, one object only should under such circumstances be seen. Again, in many cases which I have already explained, the simultaneous affection of the two retinae excites a different idea in the mind to that consequent on either of the single impressions, the latter giving rise to the idea of a representation on a plane surface, the former to that of an object in relief; these things could not occur did we see with only one eye at a time.

Du TOUR\* held that though we might occasionally see at the same time with both eyes, yet the mind cannot be affected simultaneously by two corresponding points of the two images. He was led to this opinion by the curious facts alluded to in § 14. It would be difficult to disprove this conjecture by experiment; but all that the experiments adduced in its favour, and others relating to the disappearance of objects to one eye really proves, is, that the mind is inattentive to impressions made on one retina when it cannot combine the impressions on the two retinae together so as to resemble the perception of some external objects; but they afford no ground whatever for supposing that the mind cannot under any circumstances attend to impressions made simultaneously on points of the two retinae, when they harmonize with each other in suggesting to the mind the same idea.

A perfectly original theory has been recently advanced by M. LEHOT,† who has endeavoured to prove, that instead of pictures on the retinae, images of three dimensions are formed in the vitreous humour which we perceive by means of nervous filaments extended thence from the retina. This theory would account for the single appearance to both eyes of objects in relief, but it would be quite insufficient to explain

\* Act. Par. 1743. M. p. 334.

† Nouvelle Théorie de la Vision, Par. 1823. .

why we perceive an object of three dimensions when two pictures of it are presented to the eyes ; according to it, also, no difference should be perceived in the relief of objects when seen by one or both eyes, which is contrary to what really happens. The proofs, besides, that we perceive external objects by means of pictures on the retinæ are so numerous and convincing, that a contrary conjecture cannot be entertained for a moment. On this account it will suffice merely to mention two other theories which place the seat of vision in the vitreous humour. VALLEE,\* without denying the existence of pictures on the retina, has advocated that we see the relief of objects by means of anterior foci on the hyaloid membrane ; and RASPAIL† has developed at considerable length the strange hypothesis, that images are neither formed in the vitreous humour nor painted on the retina, but are immediately perceived at the focus of the lenticular system of which the eye is formed.

## § 16.

It now remains to examine *why* two dissimilar pictures projected on the two retinæ give rise to the perception of an object in relief. I will not attempt at present to give the complete solution of this question, which is far from being so easy as at a first glance it may appear to be, and is indeed one of great complexity. I shall in this place merely consider the most obvious explanations which might be offered, and show their insufficiency to explain the whole of the phenomena.

It may be supposed, that we see but one point of a field of view distinctly at the same instant, the one namely to which the optic axes are directed, while all other points are seen so indistinctly, that the mind does not recognise them to be either single or double, and that the figure is appreciated by successively directing the point of convergence of the optic axes successively to a sufficient number of its points to enable us to judge accurately of its form.

That there is a degree of indistinctness in those parts of the field of view to which the eyes are not immediately directed, and which increases with the distance from that point, cannot be doubted, and it is also true that the objects thus obscurely seen are frequently doubled. In ordinary vision, it may be said, this indistinctness and duplicity is not

\* *Traité de la Science du Dessin*, Par. 1821, p. 270.

† *Nouveau Système de Chimie Organique*, t. 2. p. 329.

attended to, because the eyes shifting continually from point to point, every part of the object is successively rendered distinct; and the perception of the object is not the consequence of a single glance, during which only a small part of it is seen distinctly; but is formed from a comparison of all the pictures successively seen while the eyes were changing from one point of the object to another.

All this is in some degree true; but were it entirely so, no appearance of relief should present itself when the eyes remain intently fixed on one point of binocular image in the stereoscope. But on performing the experiment carefully, it will be found, provided the pictures do not extend too far beyond the centres of distinct vision, that the image is still seen single and in relief when this condition is fulfilled. Were the theory of corresponding points true, the appearance should be that of the superposition of the two drawings, to which however it has not the slightest similitude. The following experiments are equally decisive against this theory.

Exp. 1. Draw two lines about two inches long and inclined towards each other, as in fig. 10., on a sheet of paper, and having caused them to coincide by converging the optic axes to a point nearer than the paper, look intently on the upper end of the resultant line, without allowing the eyes to wander from it for a moment. The entire line will appear single and in its proper relief, and a pin or a piece of straight wire may without the least difficulty be made to coincide exactly in position with it; or, if while the optic axes continue to be directed to the upper and nearer end, the point of a pin be made to coincide with the lower and further end or with any intermediate point of the resultant line, the coincidence will remain exactly the same when the optic axes are moved and meet there. The eyes sometimes become fatigued, which causes the line to appear double at those parts to which the optic axes are not fixed, but in such case all appearance of relief vanishes. The same experiment may be tried with more complex figures, but the pictures should not extend too far beyond the centres of the retinae.

Another and a beautiful proof that the appearance of relief in binocular vision is an effect independent of the motions of the eyes, may be obtained by impressing on the retinae ocular spectra of the component figures. For this purpose the drawings should be formed of broad coloured lines on a ground of the complementary colour, for instance red lines on a green ground, and be viewed either in the stereoscope or in the ap-

paratus, fig. 6., as the ordinary figures are, taking care however to fix the eyes only to a single point of the compound figure; the drawings must be strongly illuminated, and after a sufficient time has elapsed to impress the spectra on the retinae, the eyes must be carefully covered to exclude all external light. A spectrum of the object in relief will then appear before the closed eyes. It is well known that a spectrum impressed on a single eye, and seen in the dark, frequently alternately appears and disappears: these alternations do not correspond in the spectra impressed on the two retinae, and hence a curious effect arises; sometimes the right eye spectrum will be seen alone, sometimes that of the left eye, and at those moments when the two appear together, the binocular spectrum will present itself in bold relief. As in this case the pictures cannot shift their places on the retinae in whatever manner the eyes be moved about, the optic axes can during the experiment only correspond with a single point of each.

When an object, or a part of an object, thus appears in relief while the optic axes are directed to a single binocular point, it is easy to see that each point of the figure that appears single is seen at the intersection of the two lines of visible direction in which it is seen by each eye separately, whether these lines of visible direction terminate at corresponding points of the two retinae or not.

But if we were to infer the converse of this, viz. that every point of an object in relief is seen by a single glance at the intersection of the lines of visible direction in which it is seen by each eye singly, we should be in error. On this supposition, objects before or beyond the intersection of the optic axes should never appear double, and we have abundant evidence that they do. The determination of the points which shall appear single seems to depend in no small degree on previous knowledge of the form we are regarding. No doubt some law or rule of vision may be discovered which shall include all the circumstances under which single vision by means of non-corresponding points occurs and is limited. I have made numerous experiments for the purpose of attaining this end, and have ascertained some of the conditions on which single and double vision depend, the consideration of which however must at present be deferred.

Sufficient, however has been shown to prove that the laws of binocular visible direction hitherto laid down are too restricted to be true. The law of AGUILONIUS assumes that objects in the plane of the horopter are alone seen single:

and the law of corresponding points carried to its necessary consequences, though these consequences were unforeseen by its first advocates, many of whom thought that it was consistent with the law of AGUILONIUS, leads to the conclusion, that no object appears single unless it is seen in a circle passing through the centres of visible direction in each eye and the point of convergence of the optic axes. Both of these are inconsistent with the single vision of objects whose points lie out of the plane in one case, and the circle in the other; and that objects do appear single under circumstances that cannot be explained by these laws, has, I think, been placed beyond doubt by the experiments I have brought forward. Should it be hereafter proved, that all points in the plane or in the circle above mentioned are seen single, and from the great indistinctness of lateral images it will be difficult to give this proof, the law must be qualified by the admission, that points out of them do not always appear double.

*A Comparative Examination of the Sugar Cane grown at the Antilles and in France, followed by Considerations on the Manufacture of Sugar.—Second Memoir.* By OSMIN HERVEY, Professor of Chemistry in the School of Pharmacy, Paris.\*

(Continued from page 64.)

#### *On the Juice of the Cane.*

THE juice of the canes which we have examined were either colourless, or of a whitish very light yellow colour, of a sweet and balsamic odour, their savour agreeable, but rather insipid, always acid to reagents.

The juice of our canes were clarified by the sole action of heat.

With alcohol a very light flocculent precipitate was formed.

The acids, whether cold or hot, clarify it, forming a light precipitate.

Lime and the alkaline carbonates clarify it equally as well; but that which is most remarkable is, that a feeble, or concentrated solution of pure caustic potash clarifies the juice of the cane, and effects it with a facility, which is still increased

\* Journal de Pharmacie, &c.

as the temperature becomes elevated : this fact proves, that the substance which congeals and produces the clarification of the juice of the cane, is neither albumen nor pectine.

Tannin, which only forms a light precipitate in the juice recently obtained, produces an abundant precipitate in the juice which has been obtained some days, and especially in the juice which has already began to thicken ; alcohol acts upon it in the same manner as tannin,

Animal carbon discolours the juice of the cane and renders it perfectly limped, without deteriorating the flavour. Thus discoloured and clarified by lamp black, the juice of the cane is no longer precipitated by tannin ; it preserves for a long time, (more than 15 days) at a temperature of  $+ 10^{\circ}$  without undergoing any alteration ; at a more elevated temperature, 18 to  $20^{\circ}$ , it undergoes, at the end of five or six days, the alcoholic fermentation, but it never thickens.

Acetate of lead produces, in the juice of the cane of France, an abundant precipitate, and the liquid which swims on its surface, clears itself at the same time ; all of which induces us to believe that the wine of that of the colonies acts, in a totally different manner with the acetate of lead, and we now proceed to show the foundations of our convictions :—All the samples of dried canes which we have examined have evolved aqueous liquids, which were always incompletely precipitated by the sub-acetate of lead : the super-incumbent liquor always remained cloudy or thick, and would not clarify even after filtration, except with the greatest difficulty. The canes of France, dried and treated in the same manner, produced liquids which became perfectly limped under the influence of the same re-agent.

The juice of the French canes, if left to itself, undergoes the alcoholic fermentation with difficulty ; we see on the contrary, that it soon thickens,—it then resembles a mucilage of gum tragacanth, and it is remarkable, that in proportion as the mass thickens the sugar diminishes, and that at last it disappears completely. We can assert, that by treating this mass with alcohol at  $85^{\circ}$ , which strongly contracts this new substance, and retains in solution the sugar which still exists as lactic acid, which is produced during this decomposition. We have observed, that sulphuric acid is opposed to this decomposition, whilst the alkalies, even the powerful ones, seem to favour it. The matter which is contracted by alcohol, washed in this vehicle, is white, soft, elastic, soluble in water, and also in acetic acid. It has a close resemblance to gum precipitated by alcohol from an aqueous solution ; it

is very abundantly precipitated by tannin, alcohol, and ether. Sulphuric acid, charcoal, and nitric acid transform it into oxalic acid.

Under the influence of a temperature from 25° to 30°, and of the yeast of beer, the alcoholic fermentation is developed, however, in the juice of the cane. But, the fact we intend to prove is, that the juice of the cane which has become foul does not undergo the viscous fermentation, and that it remains unchanged for a long period of time, whilst the raw juice of the cane readily changes, and undergoes the viscous fermentation with great facility.

Whence comes it then that molasses evolves alcohol (cold) with such great facility? This is evidently due to heat, which re-acts like charcoal on the principle of the viscous fermentation; (charcoal acts by absorbing it—heat by modifying it, by paralyzing its action); we base our opinion on the following experiments—we divided into two equal parts 100 grammes of the juice of the cane of 6° Baume; we then evaporated them at twice to a third part of their weight, the one by ebullition in a free atmosphere; the other at the ordinary temperature under a capsule, causing it to absorb water by employing caustic lime; on being brought back to their former density with distilled water: at the end of five days, at 15° temperature, fermentation was manifested already on the sucrose liquid, which had not been submitted to the action of heat, whilst the other at the end of fifteen days had not evinced any phenomena of fermentation.

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*On the Composition of Sugar of Gelatine, and of Nitro-Saccharic Acid.* By M. BOUSSINGAULT.\*

I HAVE been occupied, during two years, on the saccharine matter obtained by M. Braconnot, by causing sulphuric acid to re-act on strong glue or size. We must bear in mind that the existence of sugar of gelatine has been doubted by several chemists. In following the indications pointed out by M. Braconnot, I obtained therefrom two substances which I have denominated sugar and *leucine*; but after some essays, made with the view of fixing the composition of these two bodies, I was obliged to interrupt my researches.

\* Extracted from the *Compté rendu de l'Académie des Sciences*.



Since that time this subject has been treated of by other chemists, and the results to which their researches have conducted them, accord in some points with those which I have obtained; but on others they differ from me remarkably. As I took every precaution, in every point which depended on me, in order to give precision to my analysis, I cannot account for these discordances; further experiments will decide on which side the errors have occurred.

*Sugar of Gelatine.*

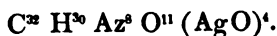
The properties of sugar of gelatine are sufficiently known by the work of M. Braconnot. Its composition, deduced from analyses made on products of divers origin, is—

	Proved.	Calculated.	
Carbon.....	33.85	34.00	C <sup>32</sup>
Hydrogen .....	6.44	6.36	H <sup>36</sup>
Azote .....	20.00	20.05	Az <sup>3</sup>
Oxygen .....	39.71	39.59	O <sup>14</sup>

By the means of some precautions indicated in my memoir, we may easily combine the sugar of gelatine with the oxide of silver. The combination presents itself under the form of colourless crystals, it is soluble in a small degree in cold water.

	Combined Sugar.	Combination.
Carbon .....	37.67	13.66
Hydrogen .....	6.12	1.21
Azote .....	22.26	8.07
Silver .....	—	63.95
Oxygen .....	33.95	12.31

This composition leads to the formula—



	Combined Sugar.	Combination.
Carbon .....	37.65	13.33
Hydrogen .....	5.86	2.08
Azote .....	22.16	7.87
Silver .....	—	64.50
Oxygen .....	34.43	12.22

The sugar of gelatine unites with much greater facility with the oxydes of lead and copper.

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These two combinations are very soluble in water. The combination with lead is obtained in a crystalline mass of an azure blue; its analysis fully conforms the deduced formula of salts of silver.

The combination with lead crystallizes in beautiful colourless needles: its solution is entirely decomposed by carbonic acid.

I have experienced some difficulty in obtaining this combination in regular proportions. The proportion of oxide of lead has several times varied from  $63\frac{1}{2}$  to  $64\frac{1}{2}$ . However, by a treatment sufficiently prolonged, there has been obtained a salt containing 64.9 gr. of oxyde, a quantity too great for the formula adopted.

SUGAR.			COMBINATION.		
	Proved.	Calculated.		Proved.	Calculated.
C .....	37.55	37.55	C .....	13.29	13.68
H .....	5.90	5.96	H .....	2.04	2.13
Az.....	22.20	22.16	Az.....	7.78	8.07
O .....	34.27	34.43	O .....	11.99	12.54
			Pb O...	64.96	63.58

#### *Nitro-Saccharic Acid.*

This acid is prepared by dissolving the sugar of gelatine in weak azotic acid. It is then slightly heated, and on cooling, the solution crystallizes; there was no observable re-action, it is really a simple solution of sugar in the acid.

Nitro-saccharic acid has a strong acid flavour, at the same time slightly sugary.

I have analysed this acid in three states; crystallized, dried to  $110^{\circ}$ , and in salts.

Dried at  $110^{\circ}$  nitro saccharic acid contains—

Proved.	Calculated.
C ..... 18.1	C <sup>32</sup> ..... 18.2
H ..... 4.2	H <sup>42</sup> ..... 4.0
Az..... 21.2	Az <sup>16</sup> ..... 21.5
O ..... 56.5	O <sup>37</sup> ..... 56.3

The nitro-saccharate of silver crystallizes very easily.

Several accidents which I had experienced in heating the nitro-saccharates of lead and of copper, made me take some precautions to decompose this nitro-saccharate; I very soon

observed, to my great surprise, that these precautions were perfectly useless. This salt of silver burns without detonation. Its composition is—

	Acid.	Salt.
Carbon .....	19.61	10.08
Hydrogen .....	3.63	1.86
Azote .....	23.01	11.83
Oxygen .....	53.75	27.63
Silver .....	—	48.60

The atomic weight which is deduced from this composition, supposing one atom of the base in the salt, is 1535.2. . But the atomic quotients evidently indicate that nitro-saccharic acid is polybasic: in fact these quotients are—

$$C \dots 8.0 \quad H \dots 8\frac{1}{4} \quad Az \dots 4.0 \quad O \dots 8\frac{1}{4} \text{ etc.}$$

The nitro-saccharate of silver becomes consequently—



Thus the dried acid at 110° loses four atoms of water, which are found to be replaced by four atoms of the oxide of silver. The analysis of nitro-saccharate of potash conducts precisely to the same consequence.

It is sufficient to glance at the formula contained in my memoir to be convinced that, in nitro-saccharic acid, azotic acid is there found not modified.

The nitro-saccharates may be represented as resulting from the union of azotic acid with the corresponding saccharate, or rather as the combination of sugar of gelatine with an azotate. We may in fact obtain the nitro-saccharates by treating the saccharates with azotic acid.

Sugar of gelatine not combined  $C^{32}H^{30}Az^8O^{14}$

Sugar in the salts.....  $C^{32}H^{30}Az^8O^{11}$

In combination with silver ...  $C^{32}H^{30}Az^8O^{11}(AgO)^4$

In combination with copper...  $C^{32}H^{30}Az^8O^{11}(CuO)^4$

In combination with lead.....  $C^{32}H^{30}Az^8O^{11}(PbO)^4$

Nitro-saccharic acid crystal...  $C^{32}H^{30}Az^8O^{11}(Ag^2O^3)^4(H^2O)^9$

Dry acid at 110° .....  $C^{32}H^{30}Az^8O^{11}(Ag^2O^3)^9(H^2O)^3$

Acid in the salts .....  $C^{32}H^{30}Az^8O^{11}(Ag^2O^3)^4(H^2O)^3$

Nitro-saccharate of silver.....  $C^{32}H^{30}Az^8O^{11}(Az^2O^3)(AgO)^4(H^2O)^2$

Nitro-saccharate of potash ...  $C^{32}H^{30}Az^8O^{11}(Ag^2O^3)(KO)^4(H^2O)^2$

*On the Evolution of Nitrogen during the growth of Plants, and the Sources from whence they derive that element.* By ROBERT RIGG. Communicated by the Rev. J. B. READE, MA., F.R.S.\*

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IN this communication I shall have the honour of submitting to the Royal Society a series of experiments, which not only confirm the principles I have already laid down in the investigations of the influence of nitrogen on the growth of plants, but also enable us to trace this important element to its source.

By what mysterious process of natural chemistry the living principle of the plant obtains this product, we must be content to remain ignorant; but to what extent a supply can be procured from the compounds upon which its agency is directed, is within the compass of legitimate experimental research.

With respect to the entire volume of nitrogen connected with the process of vegetation, it will be observed that my former Tables show that the quantity appropriated by the plant varies from  $\frac{1}{1000}$  to  $\frac{1}{2}$  of their weight of carbon; and the important experiments of Dr. DAUBENY, SAUSSURE, Sir HUMPHRY DAVY and others, have abundantly established the fact that there is a considerable evolution of nitrogen during the growth of plants. Experiments, however, upon the quantity of nitrogen evolved are so beset with difficulties, that our best efforts in this department of vegetable physiology have not enabled us to speak with certainty as to the absolute or relative quantities of the gases which are given off during the healthy action of the functions of vegetable life. And in corroboration of this remark it is only necessary to state, that the natural course of vegetation is unavoidably interrupted, by the necessity we are under of excluding plants from the free action of the external atmosphere, when we attempt to collect the gases they give off.

Sir HUMPHRY DAVY made his experiments under as favourable circumstances as the nature of the case will admit of; and he found that when the oxygen decreased in quantity about two per cent. the nitrogen increased four per cent., and that when the oxygen increased about thirty-five per cent. the nitrogen increased about seven per cent. Some of my own

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experiments have been conducted on the principle adopted by Sir HUMPHRY DAVY. I have also introduced whole branches of trees, as well as stems and leaves of more tender plants, under water, without removing them from the parent stock, and collected their gaseous products in inverted glass vessels. Upon examining these products, I find, that the gaseous volumes are perceptibly affected by the brightness of the sun's rays, though their variation appears to be subject to no fixed law. At the same time, however, it is certain that nitrogen, whether appropriated or evolved, is apparently the most uniform, while oxygen assumes the character of a most fluctuating and passive agent.

Hence, then, if plants, taken as a whole, contain from three to four per cent. of their weight of nitrogen, and if to this we add the indefinite quantity, so far as we can comprehend, which is evolved during their growth, the question naturally arises, From whence do plants draw this part of their substance, as well as the volume of nitrogen which they furnish to the atmosphere? a question of the greatest importance in the practical application of the subject, and one not wanting in interest as a part of the inquiry into the chemical changes which occur during the growth of plants.

The quantity of animal and vegetable matter which forms a part of all soils suitable for the growth of plants, invariably containing nitrogen, furnishes us at once with one source from whence they may derive a part, if not the whole, of this element, which is either found in their constitution, or given off to the atmosphere during their growth. Hence an accurate examination of these soils at different periods might enable us to determine the question as to whether or not the earth supplied the whole of the nitrogen which is employed in vegetable economy. But from the many difficulties consequent upon accurately experimenting upon soils previous to and after the production of any plants, and also the examination of the plants so produced, we are not able by this mode of proceeding to obtain results in any degree satisfactory.

In order to free myself from all doubt upon this point, and to ascertain whether *the atmosphere* did or did not furnish any part, and if any what proportion, of the nitrogen which enters into the constitution of plants, I had recourse to the ultimate analysis of seeds and young seedling plants.

Some seeds I steeped in distilled water, and some in filtered Thames river water. When they had absorbed a sufficient quantity of water to favour the first impulse of germination, I spread them on glass or china plates, and kept them at tempera-

tures favourable to germination and vegetation, in a room where the only source of nitrogen would be that which was supplied by the seed and the atmosphere. The quantity of dry seed experimented upon varied from thirty to one thousand grains, which were accurately weighed. These I allowed to germinate and grow, keeping them regularly supplied with the respective kinds of water only; and that nothing might be lost by the water carrying off any of the soluble parts of the different seeds, that water which was employed in steeping each kind of seed at the commencement was afterwards used in watering the same.

Some of the experiments I favoured so as to have them germinating and growing quickly, and others slowly. Some of them were terminated at one stage of their growth, and some at another; and some of them were allowed to vegetate as long as the seeds appeared to afford them nourishment. At the end of each experiment the whole mass was enveloped in very thin paper, and dried at a temperature varying from 100° to 110° FAHR., powdered, and afterwards allowed to acquire the hygrometric state of the atmosphere. They were accurately weighed in this condition, a portion of each part, or of the entire mass, subjected to ultimate analysis, and calculation made for the whole. Thus I was enabled to discover an increase or decrease of any of the elements in any experiment so conducted, wherein the seed itself, the water, and the atmosphere furnished whatever was required for vegetation. The experiments which have reference to this part of the subject are found in Table I.

We are led by these experiments to the inference, that independent of that indefinite quantity of nitrogen which is given off to the atmosphere during vegetation, there is an increase of this element in plants when compared with its quantity in the seeds; and in this case the seeds form the only source from whence they could derive it, with the exception of the atmosphere and any little which might have combined with the water used on the occasion.

It would be at variance with my mode of research, which is purely experimental, to make any observations upon the quantity of nitrogen which is probably furnished by the atmosphere during the germination of seeds and the full growth and development of plants. The experiments before us dispose us to infer that it differs with the temperature at which the plants are exposed, and with exposure as regards sunshine and the shade. Thus we have in the germination and vegetation of barley, for instance, the quantity of nitrogen

in the grain germinated under very favourable circumstances for the process, increasing to an extent equal to thirty-eight per cent. upon the original quantity contained in the seed: when the same kind of barley was kept under unfavourable circumstances for vegetation, and allowed to grow until the principal part of the farina was exhausted, the increase in the quantity of nitrogen was only eighteen per cent. When the same grain was allowed to vegetate in the sun's rays until about two-thirds of the flour contained in the seed had disappeared, the increase in the quantity of nitrogen was thirty per cent.; and when the same plants were kept under the most favourable circumstances, and allowed to vegetate until the seeds appeared to be exhausted, the young plant during this time having the most healthy appearance, there was an increase upon the quantity of nitrogen contained in the seeds of *nearly fifty per cent.*

Seeds of cress during vegetation increased their quantity of nitrogen forty-one per cent. when the plants were kept under a temperature varying with shade and sunshine from 60° to 84°. The experiments upon the seeds of the turnip, an important plant in an agricultural point of view, (about one half of which germinated,) shows that the nitrogen which was derived from the atmosphere was more than that which was contained in the seed. In all these experiments we have the quickness of the growth of the young plants proportional to the quantity of nitrogen present when compared with 1000 parts of carbon in the same.

With these facts before us, we are enabled to account for plants not continuing to grow so well in pure oxygen gas as in atmospheric air; and by following up the inquiry in other departments, we see the wisdom of the all-wise Contriver in constituting an atmosphere with a decided preponderance of nitrogen and a much smaller proportion of oxygen.

Finding that plants under different circumstances of growth differ in the relative quantity of nitrogen which they contain, as well as in the other elements which enter into their constitution, I was desirous of obtaining information which would lead us to account for the well-known fact of plants increasing most in size during cloudy weather, and of grasses, for instance, which are shaded (as under hedges), increasing much in straw, but producing seed both small in quantity and inferior in quality.

By way of commencement in this part of the research, I made duplicate experiments with the same kind of seeds,

steeped them in and supplied them with the same kind of water, and kept them under equal circumstances in every respect, except that of placing one of each in the sun's rays in the greenhouse, where the sun shone till three P. M., and the other in the same situation, excluded however from the sun's rays, but not from the light. Those in the shade increased in length much more than the others, were a little lighter in colour, and when weighed before they were exposed to the drying temperature, were also heavier, but when dried at 100° to 110° were considerably less in weight. The ultimate analyses of these experiments are found in Table II., and the result of the experiments as regards nitrogen is, that those plants which weighed the heaviest before drying, and which had as it were moulded into shape the largest quantity of matter in the form of plants, contained, in the cress for instance, 147 parts of nitrogen for every 1000 parts of carbon; whereas those plants from seed of the same kind which grew in the sun's rays, and whose weight before drying and when freed from foreign water was nine per cent. lighter than the other, contained only 111 parts of nitrogen for 1000 of carbon;—and in addition I might make an observation which is in perfect harmony with all that has been noticed upon the influence of nitrogen on the growth of plants, viz. that whereas the cress arrived at its state of maturity, so far as the seed could furnish it with the nourishment, in eighteen days, the rape had not exhausted all its seed in twenty-six days; and the proportionate quantity of nitrogen in those plants was, in cress in the sun's rays 111, in rape in the same situation 73; and in cress in the shade 147, and in rape 82, when compared with 1000 parts by weight of carbon in each.

In concluding this subject, upon which I have been as brief as its nature would admit of, not even entering at all upon the practical application thereof, the point of view wherein its real value consists, I beg to observe that, although nitrogen appears from these experiments to be a very powerful agent in the economy of plants, it is far from my intention to give it any undue importance. It is my object to draw attention to an element which, comparatively speaking, has escaped unnoticed, and to vindicate the necessity of a most scrupulous attention to those products which, though so minute in quantity as to be with difficulty detected in our balances, have nevertheless been wisely assigned to discharge the most important functions.



TABLE I.

		Carbon.	Hydr.	Oxyg.	Nitr.	Resid.	Water.	Total.	Nitr for 1000 Carb.
Barley steeped in distilled water germinated quickly until the plumula of several seeds had passed through the grain.	The malt ....	35.14	....	1.51	1.73	1.15	44.67 =	84.2	49
	The rootlets and plumula }	1.70	0.2	....	.18	.16	2.14 =	4.2	106
Barley in its original state.....		36.84 39.57	0.2 ....	1.51 3.45	1.91 1.38	1.31 1.30	46.58 = 54.3 =	88.4 100	35
Increase .....		.....	0.2	....	.53	.01			
Decrease .....		2.73	....	1.94	....	....	7.72 =	11.6	
Barley steeped in river water, vegetated in the shade until the corn appeared to be ex- hausted.	The stems ..	7.98	.17	....	.61	.86	11.38 =	21.0	76
	The roots....	8.90	....	....	.72	1.00	13.37 =	23.99	81
	The husks ..	6.19	....	.25	.26	.53	7.27 =	14.5	24
	The liquid separated by drying }	.04	.01	....	.02	....	=	.07	
Barley in its original state.....		23.11 39.57	.18 ....	.25 3.45	1.61 1.38	2.39 1.30	32.02 = 54.3 =	59.56 100	35
Increase .....		.....	.18	....	.23	1.09			
Decrease .....		16.46	....	3.2	....	....	22.28 =	40.46	
Barley steeped in distilled wa- ter, and vegetated until about two-thirds of the flour had disappeared; kept in the sun's rays.	Original state	39.57	....	3.45	1.38	1.30	54.3 =	100	35
	The vegeta- ted mass }	29.7	.5	....	1.80	1.3	44.3 =	77.6	60
	Increase ....	.....	.5	....	.42	....			25
	Decrease ....	9.87	....	3.45	....	....	9.7 =	22.4	
Barley steeped in rain water grew until the stems were five and six inches long; kept in the sun's rays.	Original state	39.57	....	3.45	1.38	1.30	54.3 =	100	35
	Young plants	27.83	....	4.13	2.06	1.36	33.82 =	69.2	70
	Increase ....	.....	....	.68	.68	.06			35
	Decrease ....	11.74	....	....	....	....	20.48 =	31.8	
Cress seed supplied with dis- tilled water, and kept in the sun's rays.	The seed ....	46.77	1.53	....	3.27	4.8	43.63 =	100	71
	Young plants	37.59	.12	....	4.64	4.75	46.10 =	93.2	121
	Increase ....	.....	....	....	1.37	....	2.47	.....	50
	Decrease ....	9.18	1.41	....	....	0.05		6.8	
The same seed supplied with river water, and kept more in the shade.	The seed ....	46.77	1.53	....	3.27	4.	43.63 =	100	71
	Young plants	33.9	.80	....	4.1	4.8	40.9 =	84.6	121
	Increase ....	.....	....	....	.83	.1			50
	Decrease ....	12.85	.73	....	....	....	2.73 =	15.4	
Turnip seed supplied with river water, about half of which germinated.	The seed ....	55.48	3.45	....	3.55	3.1	34.42 =	100	65
	After vegetat.	40.70	1.82	....	4.48	4.48	32.62 =	84.5	109
	Increase ....	.....	....	....	.93	1.38			44
	Decrease ....	4.78	1.63	....	....	....	1.8 =	15.5	
Rape seed, about two-fifths of which grew.	The seed ....	55.29	3.45	....	2.71	3.1	35.45 =	100	50
	After vegetat.	44.31	1.58	....	3.14	3.1	40.37 =	92.5	73
	Increase ....	.....	....	....	.43	....	4.92		23
	Decrease ....	10.98	1.87	....	....	....		7.5	

TABLE II.

		Carbon.	Hydr.	Oxyg.	Nitr.	Resid.	Water.	Total.	Nitr. for 100 Carb.
Cress seed supplied with river water, and kept in the sun's rays in the greenhouse.	The seed ....	46.77	1.53	....	3.27	4.8	43.63 = 100		71
	Young plants.	40.03	.98	....	4.44	10.5	36.35 = 92.3		111
Increase .....		.....	.....	.....	1.17	5.7	.....		40
Decrease .....		6.74	.55	.....	.....	.....	7.28 = 7.7		
The same kind of seeds, but kept in the shade.	The seeds....	46.77	1.53	....	3.27	4.8	43.63 = 100		71
	Young plants.	30.08	.92	....	4.42	9.97	33.91 = 79.3		147
Increase .....		.....	.....	.....	1.15	5.17	.....		76
Decrease .....		16.69	.61	.....	.....	.....	9.72 = 20.7		
Rape seed supplied with river water, and kept in the sun's rays.	The seeds....	55.29	3.45	....	2.71	3.1	35.45 = 100		50
	Young plants.	45.35	1.38	....	3.20	8.12	48.75 = 106.8		73
Increase .....		.....	.....	.....	0.49	5.02	13.3 = 6.8		23
Decrease .....		9.94	2.07	.....	.....	.....	.....		
The same kind of seeds kept in the shade.	Seed.....	55.29	3.45	....	2.71	3.1	35.45 = 100		50
	Young plants.	39.34	1.90	....	3.19	7.52	38.65 = 90.6		82
Increase .....		.....	.....	.....	.48	4.42	3.2 = ....		32
Decrease .....		15.95	1.55	.....	.....	.....	9.4		
Mustard seed steeped in river water; kept in the sun's rays.	Seed.....	50.74	2.36	....	3.55	3.9	39.45 = 100		70
	Young plants.	33.33	.88	....	3.98	5.0	37.51 = 80.7		119
Increase .....		.....	.....	.....	.43	1.1	.....		49
Decrease .....		17.41	1.48	.....	.....	.....	1.94 = 19.3		
The same kind of seed kept in the shade.	Seed.....	50.74	2.36	....	3.55	3.9	39.45 = 100		70
	Young plants.	31.68	1.66	....	3.93	3.85	30.21 = 71.33		128
Increase .....		.....	.....	.....	.38	.....	.....		58
Decrease .....		19.06	.70	.....	.....	.05	9.24 = 28.67		
The chemical constitution of the young plants mentioned in Table II. when brought to 100 parts are	Cress in the sun's rays	43.36	1.06	....	4.81	11.4	39.37 = 100		111
	Cress in the shade	37.92	1.16	....	5.57	12.6	42.75 = 100		147
	Rape in the sun's rays	42.46	1.29	....	3.00	7.6	45.65 = 100		73
	Rape in the shade	43.39	2.1	....	3.55	8.3	42.66 = 100		82
	Mustard in the sun's rays	41.32	1.09	....	4.93	6.2	46.46 = 100		119
	Mustard in the shade	44.41	2.32	....	5.51	5.4	42.36 = 100		128

*On the formation of CAL in the cauldrons during the fabrication of Sugar from the Cane.* By M. AVEQUIN, of New Orleans.

THERE are many inconveniences attached to the fabrication of baked sugar; one of the most serious and most perplexing of which is the formation of *Cal* in the interior of the cauldrons in which the evaporation of the juice of the cane is carried on. I am now about to endeavour to give some explanations of its formation and of its nature.

In the work of a sugar manufactory, when one baking apparatus is made use of during several days continuously, there will be formed in the interior of the cauldrons an encrustation of plaster which adheres tenaciously to their sides, and which augments progressively in thickness to such an extent that the apparatus can no longer be used, until the encrustation be removed. This crust sometimes acquires a thickness of three or four lines, and considerably hinders or retards the ebullition of the juice of the canes, or syrup, forasmuch as this plaster is a very bad conductor of heat. Amongst the manufacturers of sugar, this stratum of plaster bears the name of *Cal*. A cauldron is known to contain *Cal*, when a hissing or whistling noise is heard proceeding from the bottom of the cauldron, and which is manifested more particularly on decanting the liquid from the cauldron. This noise is produced by cracks or fissures which are formed in the *Cal* by the action of heat, and the syrup or liquid insinuating itself through the crack, is brought into contact with the metallic surface of the cauldron, which is of a much higher temperature than itself; this gives rise to the production of puffs or blasts of white vapour, occasioned by the decomposition of a small quantity of sugar. These puffs of white vapour are certain indications of the presence of *Cal*.

The formation of *Cal* in the sugar cauldrons is due to the presence of the bi-sulphate of lime existing naturally in great quantities in the juice of the canes; by adding milk of lime in order to obtain its clarification, the excess of the acid of the biphosphate becomes saturated by this base; there results from this an insoluble sub-phosphate, which presents itself in large flocks of a grayish colour, slightly charged with colouring matters, and some light remains of the cane. These flocks float in the juice of the canes, or in the syrup, when the clarification has not been fully complete or the liquid has not been skimmed, or has not been done at the opportune moment; and when these flocks happen to touch the sides of

the cauldrons, they adhere strongly to them, nor do they again remove from them. This stratum augments its thickness by degrees, as I have just stated above, and finally becomes very prejudicial to the concentration of the syrup, because this plaster or cement is a very bad conductor of heat. No means have at present been discovered to oppose or prevent its formation.

In order to remove the *Cal* from a cauldron, the workmen are obliged, as they say, to burn it. This operation is performed sometimes without interfering with the working of the apparatus; in order to do this, they profit by the moment of the decantation of the clarified juice, when the cauldron is empty; they leave it in this state, exposed to a violent heat for a few seconds, or until the bottom of the vessel has attained a dull red heat; all the stratum of plaster which forms the *Cal*, undergoes the first stage of carbonization, detaching itself from the sides of the cauldron very easily, and falling in large cakes, or by crusts, which are sometimes from two to three inches in diameter. This takes place more particularly in the clarifying cauldron, that is to say, in that which is farthest removed from the heat. In the battery or baking cauldron, the *Cal* is ordinarily in part reduced to charcoal of a red or brown colour, according as the cauldron has been more or less heated; in order to detach the *Cal*, they withdraw quickly as great a quantity of the syrup as possible, either with a skimmer, or a spout (*pucheux*) of copper; they then throw into the cauldron as quick as possible a bucket of cold juice of the cane, rinsing it well, and with as much expedition as possible, fill it again with the syrup or the juice of canes, and the work proceeds as before. Most frequently, however, the *Cal* is not withdrawn except the process is stopping for some hours, and in the course of a season these occasions frequently occur. This latter means is infinitely preferable, inasmuch as it gives time to perform it with ease, and presents less danger of breaking or damaging the cauldrons. It would be impossible to detach the *Cal* from a cauldron if these means were not employed; it adheres with such tenacity, that the graving chisel and hammer are, so to speak, of no effect.

This *Cal* is a serious inconvenience; its formation is a most perplexing accident, as it deprives the cauldron in a great measure of the property of conducting heat; and when cast iron cauldrons are made use of, they are frequently broken. These accidents seldom happen, however, with wrought iron or copper cauldrons, because these metals are capable of undergoing a quick expansion and contraction of their parts,

without any ill effects ; it is well known that the properties of cast iron are very different. In expounding the fabrication of sugar, I shall explain a means of detaching it with much facility, and without being obliged to burn the cauldrons so strongly ; by these simple means, which have succeeded with me exceedingly well, the *Cal* is separated in plates or crusts, which are sometimes more than a foot in diameter.

### *Examination of CAL.*

One thousand grammes of *Cal*, after having been withdrawn from the clarifying cauldron, as it is called the highest, has been heated by a moderate heat in a Hessian crucible, till its incineration has been perfect. At the commencement of its combustion, a large quantity of carburetted hydrogen gas was disengaged, which burnt in the crucible with a bluish white flame : as the combustion advanced the white flame disappeared, and was replaced by a violet or blue flame. The *Cal* must be kept for a long time at a light red heat before a perfect incineration is effected. These 1,000 grammes were reduced to 492 grammes, after a calcination of twenty-four hours.

One hundred grammes of *Cal* calcined as above stated, after being powdered very fine, was treated with nitric acid in excess, and diluted with four parts water ; a light effervescence ensued ; a great portion of this matter was dissolved, the other remained in the form of a powder having a tint of light gray. After a contact of twelve hours, the whole was diffused through 500 grammes of pure water, thrown on a filter, then carefully washed in boiling water, and dried. As the residue unattacked by the nitric acid, appeared to me not to be perfectly incinerated, I calcined it a second time ; it was again treated with nitric acid, in great excess, by which the greater part of this residue was dissolved. The small portion yet unattacked was carefully washed in boiling water, and thrown on to a filter. After perfect dessication, it was exposed for ten minutes to a red heat in a platinum crucible ; the residue was almost white, and was not attacked by the powerful acids. Ground with four times its weight of caustic potash, and dissolved in water, it was precipitated by treating the solution with nitric or hydrochloric acid ; on evaporating it to dryness, dissolving it in water, and filtering it, there remained a white powder, which after being heated to redness, appeared quite unattackable by the most powerful acids. It weighed 4.70 grammes.

The nitric solution, and the waters which had been used

for washing the residue as above stated, were mixed, and conveniently brought together; I poured ammoniacal liquid into it until precipitation ceased. The ammonia having been added in excess, the solution took a well grounded green colour; the precipitate which the ammonia had thrown down, was very white, and gelatinous; after being washed in boiling water with great care, and submitted to a perfect dessication, at a temperature of 100°, it weighed 112 grammes; by a calcination to a red heat these 112 grammes were reduced to 92.43 grammes.

This matter was entirely insoluble in water; treated by nitric or hydrochloric acid diluted with water, it dissolved afterwards without effervescence; essayed with nitrate of silver it instantaneously took a beautiful canary yellow colour. This precipitate is evidently a sub-phosphate of lime.

The nitric solution, from which the calcareous sub-phosphate just mentioned had been separated, was treated by a solution of sub-carbonate of potash; which gave rise to a precipitate of carbonate of lime, weighing 1.35 grammes after repeated washings, and a perfect dessication.

The nitric solution from which the precipitates of sub-phosphate and calcareous carbonate had been separated, was treated by passing it through a current of hydro-sulphuric acid; from which there resulted a precipitate of the bi-sulphate of copper, which, after having been collected, washed and converted into a deutoxide by calcination, weighed 0.75 gramme.

Of the phosphate of copper 1.41 gramme.

Thus the *Cal* which forms in the cauldrons during the concentration of the juice of the cane, is composed chiefly of sub-phosphate of lime, a small quantity of silica, sugar and mucilaginous matter in small quantity, of lime a part in the state of a carbonate, accruing from the milk of lime which is added in order to obtain the clarification of the cane juice, and a very small portion of phosphate of copper, if the sugar has been made in a copper equipage, but which is not found if the cauldrons are made of cast or wrought iron.

One hundred parts of *Cal*, after having been calcined in order to destroy the sucreous matter, and other vegetable matters, presents nearly the following composition:—

Sub-phosphate of lime.....	92.43
Lime partly carbonated .....	1.35
Silica.....	4.70
Phosphate of copper.....	1.41

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99.89

*On the Compounds of Hydrated Sulphuric Acid, with Deutoxide of Nitrogen.* By ADOLPH ROSE.\*

OF late, the sulphuric acid of commerce to be met with is more or less contaminated by one of the oxygenous compounds of nitrogen, and becomes more or less of a dark red or deep brown colour, by the addition of proto-sulphate of iron. We generally attribute this circumstance to the presence of nitric acid, which, partly through decomposition of the white crystals which are frequently, or almost always, formed in the leaden chambers; and partly through direct oxydation of the hyponitrous acid which originates in those chambers. According to GAULTIER DE CLAUDE, and HENRY, these crystals consist of sulphuric acid, hyponitrous acid, and water: and when dissolved in water become decomposed, and resolved into sulphuric acid, nitric acid, and disengaged deutoxide of nitrogen.

The nitric acid, when even in small portions in the mercantile English sulphuric acid, is considered very prejudicial in the preparation of hydrochloric acid, because it always occasions the formation of chloro-hydrochloric acid, (*Chlorhaltige Salzsäure*); or, a (*Chlorine holding hydrochloric acid*); or, hydrochloric acid holding chlorine in solution.†

In order to purify the sulphuric acid from nitric acid and nitrate of lead, it is generally submitted to distillation. By distilling sulphuric acid highly coloured with proto-sulphate of iron, and permitting some ounces of liquid to pass over, I removed the receiver, in which, as is stated in all works on this subject, the water and nitric acid ought to be contained. To my surprise, however, I found that this liquid, as well as that afterwards distilled over, was quite free from every trace of either nitric acid, or any other oxygenous compound of nitrogen; whereas, the residuum in the retort indicated more than at first: a fact, which I afterwards found had been already observed by BARRUELL, and confirmed by WACKENRODER.‡ By another distillation, in which I employed four pounds of the same kind of acid, I obtained a similar result; and obtained something more than a pound and a half of entirely pure acid.

I afterwards mixed four pounds of sulphuric acid with four ounces of nitric acid of the specific gravity 1.4, and sub-

\* (Poggendorff's *Annalen der Physik und Chemie*, Bd. L. Stuck 1.)

† The German phrase, *Chlorhaltige Salzsäure*, is very comprehensive and definite; and might very well be Englished by "*Chloro-hydrochloric acid*," or by "*Chloro-muriatic acid*."—TRANS.

‡ *Central Blatt.*, 836. 314.

mitted the mixture to distillation, using only a thin layer at once, in order that I might have an opportunity of frequently removing the receiver. The first three ounces that distilled over were very watery, and contained much nitric acid and but little of the sulphuric. Afterwards passed over two or three ounces of sulphuric, and containing but little of the nitric. The next two ounces that passed over contained only a trace of nitric acid; after which, a pound and some ounces were distilled over, of quite pure sulphuric acid. Afterwards, an acid was distilled over containing traces of the oxygenous compounds of nitrogen, on the appearance of which the process was terminated.

The residuum in the retort was of a yellowish colour, and, when mixed with water, developed deutoxide of nitrogen, which when in contact with atmospheric air, became converted into nitrous acid vapour. In order to ascertain this circumstance still more decidedly, I introduced some of the acid to beneath an inverted cylinder, filled with distilled water, which was soon filled with a colourless gas: and, on mixing this gas with air, it became red—and was consequently deutoxide of nitrogen. The residue behaved towards re-agents generally, in the same manner as the solution of anhydrous sulphate of deutoxide of nitrogen in sulphuric acid;\* from which circumstance I inferred that it might possibly be of a similar composition; and this view has, by subsequent experiments, been completely verified.

In order to be certain that the residue from the distillation, did really contain deutoxide of nitrogen, and not, as also had been stated, any hyponitrous acid, or any nitric acid, I diluted half an ounce of it sufficiently with water to carry off all the deutoxide of nitrogen. The liquid was then divided into two equal parts, into one of which I put a drop of nitric acid; both halves were then boiled for the same length of time, which was but a few minutes. That portion to which I had added the nitric acid assumed a very dark colour by adding to it a solution of protosulphate of iron in pure sulphuric acid, whilst the other portion, on the contrary, acquired no colour by a similar treatment. Had the residuum contained hyponitrous acid, the dilution by water must have been productive of nitric acid; and I should have had in the boiled liquid a re-action of that acid manifested. I have since proved that this process is the best hitherto discovered for ascertaining the presence or absence, as the case may be, of deutoxide of nitrogen, or of hyponitrous acid, in sulphuric acid.

\* Henrich Rose. Poggendorff's Annaler, Bd. xxxviii. S. 605.



Proto-sulphate of iron is an excellent re-agent for detecting the slightest traces of deutoxide of nitrogen, hyponitrous acid and nitric acid. Nevertheless, it is absolutely necessary in all cases to add to the liquid to be examined, a sufficient quantity of pure sulphuric acid: otherwise small traces of those impurities may easily be overlooked, and remain undetected.

Deutoxide of nitrogen and hyponitrous acid are easily distinguished from nitric acid, when met with in concentrated sulphuric acid, by adding to it a dilute solution of bichromate of potassa; they reduce the chromic acid into protoxide of chromium, and become transformed into nitric acid. The liquid then assumes a green colour; but it is necessary that the dilute solution of bichromate of potassa be added drop by drop, otherwise the green tint becomes concealed by an excess of the re-agent. But the nitric acid cannot reduce the bichromate of potassa, by operating on the chromic acid. A solution of permanganate of potassa cannot be applied for this purpose, because it becomes altered by the action of strong acids; but it is a very superior re-agent, if the sulphuric acid be previously diluted with about six times its weight of water. If, indeed, we mix sulphuric acid with pure distilled nitric acid, and then dilute the mixture with six parts water, we may, after cooling, add a few drops of the solution of permanganate of potassa to the liquid, and yet no discolouration will take place. On the other hand, however, when I diluted with about six parts of water, the residuum left from distillation, which, as I have made probable, consists of a solution of the deutoxide of nitrogen in sulphuric acid, the solution of permanganate of potassa discoloured it. Again, if I heat the diluted liquid, only for a moment, a solution of the proto-sulphate of iron still indicates the presence of deutoxide of nitrogen, so as even yet to become discoloured by a solution of permanganate of potassa. But if the liquid be heated for a longer time, the solution of proto-sulphate of iron in sulphuric acid no longer tends to produce colour; for a drop of the solution of permanganate of potassa produces no colour whatever.

M. WACKENRODER\* proposed to add to the sulphuric acid, either concentrated or diluted, a solution of deuto-sulphate of manganese, which will not become discoloured by the presence of nitric acid, but immediately by deutoxide of nitrogen and hyponitrous acid. However, the solution of permanganate of potassa is a much better test.

\* Annalen der Pharmacie, Bd. xviii. s. 154.

A portion of the residuum from the distillation of a mixture of sulphuric and nitric acids was placed in a small retort, and distilled over a spirit lamp, with the intention of ascertaining correctly the character of the products which passed over to the receiver. The first portion that came over was sulphuric acid, containing a very little deutoxide of nitrogen; however, the latter increased in quantity more and more as the distillation proceeded; and the last distilled liquor was a concentrated solution of the sulphate of the deutoxide of nitrogen in sulphuric acid, which behaves towards re-agents in the same manner as a solution of another similar compound that I shall presently mention. In the retort there remained behind only a small trace of sulphate of lead. The last distilled portion was white, turned yellow by heating, and green by dilution with water, then blue, and finally became colourless, from the liberation of much deutoxide of nitrogen.

I have sought for a *direct* method of forming compounds of deutoxide of nitrogen and hydrated sulphuric acid. When deutoxide of nitrogen, dried over chloride of calcium, is conducted into English distilled sulphuric acid, contained in a large vessel, and prevented from coming into contact with air, a rapid absorption of gas takes place, and not a bubble is seen to escape; but when the evolution is too brisk, the escape of unabsorbed deutoxide of nitrogen is observed. It forms a crystalline carbonization on the sides of the vessel; which, however, dissolves in the acid and entirely disappears when the whole has been well shaken. By a longer continuation of the gas, the liquid assumes a lilac colour, then light blue, and eventually a fine deep blue, and that without any observed increase of temperature. The fluid always becomes thick by agitation, and flows like a thick syrup down the sides of the vessel containing it. By agitation it becomes converted into a white frothy mass, which by subsequent repose, dissolves into a bluish syrup. By continuing the current of gas still further, the liquid becomes transformed into a white crystalline mass, which by a gentle heat melts without decomposition, and by cooling again, hardens.

Were not the deutoxide of nitrogen dried over chloride of calcium, prior to its being conveyed into concentrated sulphuric acid, a considerable development of heat would take place. The crystals, however, appear to be formed with greater promptitude; so that we would be led to infer that a small quantity of steam is favourable to their formation.

If the crystalline mass be placed in a dry glass vessel, and water be gradually added to it, deutoxide of nitrogen becomes

disengaged, and there is produced, accordingly with the quantity of water introduced, a green, blue, and finally a colourless liquid. In concentrated sulphuric acid the crystalline mass dissolves without alteration of colour; and if the solution be submitted to distillation, the excess of sulphuric acid first passes over with an admixture of some sulphate of deutoxide of nitrogen; and after that, there distils over a concentrated solution of the sulphate of deutoxide of nitrogen, which is not decomposed by several future distillations. This solution, and also the crystalline mass has been examined for the purpose of detecting the presence of nitric acid and hyponitrous acid, but no trace of either has been found. The following experiment of comparison was necessary in this place. I separated the diluted solution into two equal parts, into one of which I introduced a drop of nitric acid, but none into the other portion; both were then boiled for the same length of time. That which contained the nitric acid, very naturally became of a deep brown colour by the introduction of sulphuric acid and proto-sulphate of iron; but the other portion did not change colour by similar additions.

The sulphate of deutoxide of nitrogen will as easily decompose by water, as a solution of the same compound in sulphuric acid: however, the decomposition does not appear so complete in the cold. Every trace of deutoxide of nitrogen may be expelled by diluting the solution with much water, and boiling for a long time. As long as an oxide of nitrogen can be detected by sulphuric acid and proto-sulphate of iron, the liquid will become coloured by a solution of permanganate of potassa.

The compound of deutoxide of nitrogen with anhydrous sulphuric acid, first represented by H. ROSE, acts in precisely the same manner as that which is the subject of my experiments. By dilution with water, deutoxide of nitrogen becomes liberated, and the same shades of colour are observed to take place: and if the diluted solution be boiled for one moment, the whole of the gas escapes; and the boiled liquid, when tested by a solution of proto-sulphate of iron in sulphuric acid, appears quite free from nitric acid. In this case also, the sulphate of deutoxide of nitrogen is more completely decomposed by the presence of sulphuric acid. If the crystalline mass be diluted with much water, and, without sulphuric acid, heated for a moment, the solution still becomes coloured by adding a solution of proto-sulphate of iron in sulphuric acid; but in this case, the cold solution also becomes

coloured by a dilute solution of permanganate of potassa. When this, by long boiling, and after cooling is no longer discoloured; no re-action is produced by a solution of proto-sulphate of iron in sulphuric acid.

These crystals, then, operate in the same manner as those which are produced in the formation of sulphuric acid, or as those which are formed by introducing hyponitrous acid to hydrated sulphuric acid: and which, as already mentioned, do not consist of deutoxide of nitrogen, sulphuric acid and water; but of hyponitrous acid, sulphuric acid and water.

According to this view, I passed into a spacious flask containing one ounce of distilled sulphuric acid, and connected air-tight with a pneumatic trough, a portion of hyponitrous acid; which I obtained by boiling one part of fecula with ten parts of nitric acid, (according to Liebig's method). The air in the flask immediately became of a deep red colour; and the disengaged gas which was collected consisted principally of carbonic acid, which is a constant attendant on hyponitrous acid when proceeding from the operation of nitric acid on fecula. As my expectation, that oxygen would be developed, was not realized, I discontinued the experiment after half an hour's operation, and found that a yellowish green fluid had been formed. When a portion of this liquid was diluted with a large proportion of water, it liberated a considerable quantity of deutoxide of nitrogen. The diluted liquid was boiled for a long time, partly with, and partly without, sulphuric acid; and to supply waste, diluting water was added from time to time: but, notwithstanding all that, the addition of a solution, of proto-sulphate of iron in sulphuric acid, produced a deep brown colour, whereas the solution of permanganate of potassa was not coloured by it.

A portion of the liquid was afterwards placed in a well stopped flask, and after being well shaken, was permitted to stand for a few hours. A white crystalline mass became formed, which separated itself from a yellowish liquid by occupying the lower place. The latter was separated from the crystalline mass by decantation through a glass funnel, and the crystals spread upon a brick and dried over sulphuric acid. The filtered liquor contained much sulphate of deutoxide of nitrogen in solution with the sulphuric and nitric acids. When this liquid was copiously diluted with water, and long boiled; (having occasional additions of water in lieu of that evaporated.) I produced, by the application of a solution of protoxide of iron in sulphuric acid, a very deep brown colour: but no discolouration took place by placing it in contact with a

dilute solution of permanganate of potassa. By dissolution in water, the previously dried crystals liberated deutoxide of nitrogen, with the beforementioned play of colours ; they act, indeed, like the sulphate of deutoxide of nitrogen, but with this difference, that in the boiled aqueous solution there are always certain traces of deutoxide of nitrogen to be found. It is probable, however, that this small trace originates in the adhering mother liquor, from which the crystals cannot easily be freed. It appears that hyponitrous acid, by transmission through sulphuric acid, becomes transformed into deutoxide of nitrogen and nitric acid. The former combines with the sulphuric acid, whilst the nitric acid in great quantity remains in the mother liquor. This may probably account for the fact, that crystals thus formed contain either nitric acid or deutoxide of nitrogen.

I have also produced similar crystals by placing sulphuric acid and deutoxide of nitrogen in a large flask holding atmospheric air, into which I could pour water and blow additional air through a glass tube. By the presence of water and an excess of deutoxide of nitrogen, the crystals became immediately formed, which, in crystalline snowy spangles partly adhered to the sides of the flask, and partly appeared in the middle of it ; placed in water they became decomposed, and liberated much deutoxide of nitrogen. When this solution was boiled with sulphuric acid and water, and then mixed with proto-sulphate of iron and pure sulphuric acid, no signs of the presence of nitric acid was manifested ; although such manifestation would certainly have taken place, had the crystals been composed of hyponitrous acid, sulphuric acid and water. To set this point at rest, I divided the solution into two parts, and with one of them I mixed a trace of nitric acid, and boiled it for a long time, and an obvious re-action of the nitric acid was produced ; but the other portion gave no such indications. Even here, have I not neglected to mix the diluted boiled solution after cooling, with a solution of permanganate of potassa, which experiment has convinced me, beyond all doubt, that no nitric acid was formed by combination with the water ; and also that these crystals do not consist of hyponitrous acid and sulphuric acid, but are a compound of sulphuric acid, deutoxide of nitrogen and water.

If, however, after the operation is ended, and the flask full of deutoxide of nitrogen, we blow into it fresh atmospheric air long enough to render it colourless, we find, after

boiling, a slight but free re-action of nitric acid, which probably originated in the aqueous solution of hyponitrous acid contained in the flask. The re-actions in this case were accomplished in the same manner as in the preceding ones.

These crystals are of the same kind as those which are formed in the leaden chambers, during the preparation of English sulphuric acid, and which are always produced when there is present an excess of deutoxide of nitrogen, with reference to the atmospheric air and sulphurous acid. In this case, a part only of the deutoxide of nitrogen is converted into hyponitrous acid; which oxidizes the sulphurous acid, and converts it into sulphuric acid; and the whole of this latter acid combines with the remaining portion of the deutoxide of nitrogen. Moreover, these crystals would also be formed by the presence of an excess of hyponitrous acid or atmospheric air, because the pre-formed sulphuric acid would resolve the hyponitrous acid into nitrous acid and deutoxide of nitrogen, with the latter of which it would combine. When it is required that these crystals should not be formed in the leaden chambers during the formation of sulphuric acid, a sufficiency of sulphurous acid should always be present.

The quantitative analysis of these crystals, and also of those which are produced by the transmission of deutoxide of nitrogen, being attended with some difficulties, I shall defer a detail of it till a future memoir.

The solution of the sulphate of deutoxide of nitrogen in sulphuric acid operates so analogous to the red fuming nitric acid, that it is not improbable, that the latter ought to be considered as a solution of the nitrate of deutoxide of nitrogen in nitric acid; an opinion which is well supported by a fact mentioned by GAY LUSSAC, viz., that a mixture of red fuming nitric acid with fuming sulphuric acid produce crystals, which, without doubt, are the sulphate of deutoxide of nitrogen. If we distil a solution of sulphate of deutoxide of nitrogen in sulphuric acid, there passes over, as already mentioned, first an excess of sulphuric acid, and afterwards the remaining concentrated solution, which will not decompose though distilled many times. By heat this concentrated solution became yellow: diluted with water it liberated deutoxide of nitrogen; and like fuming nitric acid, it assumed the green or blue tints, or became perfectly colourless, accordingly with the quantity of water added. Unassisted

by heat, however, the mere dilution with water will not effect its decomposition, which is also the case with the red fuming nitric acid: but diluted solutions of both, even after long keeping, discolours the solution of permanganate of potassa. By heat, however, both solutions part with their deutoxide of nitrogen, but most freely the fuming nitric acid; but both liquids may be again charged with that gas by merely conducting it into them.

BERZELIUS\* considers it quite as probable that, fuming nitric acid is a compound of deutoxide of nitrogen and nitric acid, as that it is one of hyponitrous acid and nitric acid, with the latter in excess; and he is borne out in this opinion by the fact that, an analogous compound exists with regard to sulphuric acid.

It appeared to me, at this time, somewhat interesting to understand how it happens that sulphuric acid, at present, is so often adulterated with sulphate of deutoxide of nitrogen, and that the nitric acid is but seldom found in it. For this purpose I diluted pure English sulphuric acid with water, till it had a specific gravity of 1.2, (the usual density when the acid leaves the leaden chambers), and mixed one portion of it with sulphate of deutoxide of nitrogen; with another portion was mixed the same sulphate dissolved in sulphuric acid; to a third portion I added pure sulphuric acid, and to a fourth, fuming nitric acid. I then heated each portion separately in retorts till the sulphuric acid had distilled over. The residue always contained pure sulphuric acid. The portion to which nitric acid was added, I had to heat so long, that the sulphuric acid left in the residuum had a specific gravity of 1.84. Also, when concentrated sulphuric acid is mixed with nitric acid, and the mixture submitted to a gentle heat, almost pure sulphuric acid is found in the residuum. If we mix the two pure concentrated acids with one another, so that no heat is produced, and let the mixture stand for several weeks, it appears to undergo no decomposition. If considerable quantities be mixed rapidly, a trace of deutoxide of nitrogen becomes formed, which is probably occasioned by the heat arising from the mixture. If the mixture be rapidly heated in a retort, a decomposition takes place; the neck of the retort becomes filled with a red vapour, and there distils over,—first, sulphuric acid charged with the nitric, and afterwards pure sulphuric acid. In the residue there remains

\* Berzelius's *Lehrbuch der Chemie*, ii. Th. s. 40.

sulphuric acid, holding deutoxide of nitrogen in solution. If the sulphuric acid be coloured by organic substances, and that it be discoloured by heat and a few drops of nitric acid, it will then be found containing sulphate of deutoxide of nitrogen.

From this, it is obvious, that sulphuric acid of commerce ought to be free from every oxygenous compound of nitrogen, when of the specific gravity of 1.84, of which strength it is but seldom found; and it can contain sulphate of deutoxide of nitrogen only, when discoloured by nitric acid. Of late years sulphuric acid has been concentrated in platinum alembics, which are so arranged, that the weak acid continually flows to that which is more concentrated, which is probably the reason that sulphate of deutoxide of nitrogen is formed, and is not afterwards distilled off.

BARRUELS\* has proposed to heat the impure sulphuric acid with sulphur to a temperature of 200°, CEN., in order to destroy the acids of nitrogen, and afterwards distil off the pure acid. But this process is superfluous, because the end is accomplished by distillation alone; for if the nitric acid should be present, the pure sulphuric acid distils over, and this is the case, as I have before observed, although a pound of sulphuric acid should contain an ounce of the nitric, a proportion which is probably never met with in commerce. But it is essential, in this process, to change the receivers frequently. Should the sulphuric acid contain deutoxide of nitrogen, even then a pure acid distils over.

In order to procure pure sulphuric acid for the preparation of hydrochloric acid; it is only necessary to mix it with two parts of water and submit the solution to distillation, for whether it previously contained sulphate of deutoxide of nitrogen, or the nitric acid, the pure sulphuric acid vapour passes over. By this process we have an additional advantage, by at once procuring an acid of the specific gravity of 1.85.

Several precautions have been recommended to prevent the occasional explosions in the neck of the retorts whilst distilling sulphuric acid; all of which, as well as the use of platinum wire, will appear useless, when proper attention is paid to the process. I have lately been in the practice of distilling sulphuric acid over the naked fire, and never yet met with any accident of the kind. It is only necessary that the neck of the retort be not too long, and that it be as wide

\* Central Blatt, 1836, s. 315.



as possible; that the receiver do not rest immediately on the neck of the retort, but prevented from touching it by a platinum wire; and that the fire be kept at an uniform heat, for which purpose charcoal is the best fuel. The retort is to be charged with the sulphuric acid to two-thirds full, and placed on a sand bath with the usual care. At first a strong heat is to be applied until thick vapours rise from the acid. The fire is afterwards to be moderated until the acid comes to a gentle boil. The heat is to be kept as equable as possible, and care taken that the boiling does not cease. Should this happen, the fire must be urged with great care, so that the acid may not be brought suddenly to turbulent boiling.

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*On the Decomposition of Ammonia by the compounds of Azote with Oxygen.* By M. J. PELOUZE.\*

WHEN, at the ordinary temperature, we bring into contact sulphuric acid with nitrate of ammonia, this salt becomes gradually dissolved, and the liquor presents no unforeseen phenomena, whatever may be the proportions or the state of concentration of the bodies composing it; that is to say, the re-agents indicate ammonia, and the sulphuric and nitric acids. Should the mixture contain water, and be submitted to distillation, we obtain on one hand all the nitric acid, and on the other all the sulphate of ammonia which theory indicates. When, on the contrary, the nitrate of ammonia, by means of heat, has been deprived of all the water that it can lose without destruction, and that it be heated with a great excess of concentrated sulphuric acid, in fifty times its weight, for example, the case is very different. At about 150° the mixture permits the disengagement of a considerable quantity of protoxide of nitrogen, and forms water which unites with the sulphuric acid: and we neither find nitric acid nor ammonia in the products of this remarkable re-action. The nitrate of ammonia under these circumstances comports itself as it does in a no less curious manner, under the sole influence of heat, and it is the only example of a nitrate which does not permit the disengagement of nitric acid by the action of the sulphuric, at the same time that it does not give up its base to that acid which is much more stable and energetic than the nitric.

\* Journal de Pharmacie, &c. May, 1841.

When the proportion of the concentrated sulphuric acid is much diminished, for example, if we operate with ten parts of that acid, and one part of nitrate of ammonia, 75 per cent of this salt becomes decomposed into nitric acid and ammonia; and the other 25 into protoxide of nitrogen and water. By diminishing the proportion of the sulphuric acid we obtain no more, or nearly no more of the protoxide of nitrogen, so that with one equivalent of nitrate of ammonia, and two equivalents of sulphuric acid, the phenomena no longer deviate from the ordinary rules of the decomposition of a salt by a more fixed acid.

These rules are also observed when, instead of carrying a mixture of nitrate of ammonia and a great excess of concentrated sulphuric acid to 160°, it is preserved between 90° and 120°. That temperature though insufficient for accomplishing the transformation of the nitrate of ammonia into water and protoxide of nitrogen, is nevertheless sufficiently high for distilling off the nitric acid eliminated by the sulphuric acid, and passing it into the receiver unaccompanied by the protoxide of nitrogen.

From the preceding facts, it obviously appears that the products of the decomposition differ very materially, accordingly with the respective proportions of the nitrate of ammonia on the sulphuric acid, the temperature of the mixture, and the quantity of the water employed.

Analogy indicated that the nitrate of ammonia ought to comport itself in this manner, and experiment has confirmed the prediction. This salt decomposed by a great quantity of concentrated sulphuric acid, aided by heat, becomes transformed into water and azote.

The deutoxide of nitrogen seems less disposed to yield to these re-actions; nevertheless, I succeed in accomplishing the decomposition with great facility, by ammonia assisted by concentrated sulphuric acid. Profiting by the observation made by M. Adolph Rose,\* that monohydrated sulphuric acid unites directly with deutoxide of azote, and absorbs very considerable quantities of that gas, I prepared this compound, and dissolved sulphate of ammonia in it, and submitted it to a temperature of about 160°. The consequence was a disengagement of pure nitrogen, without any mixture of either *protoxide* or of deutoxide of that body.

I have varied these experiments by passing deutoxide of nitrogen into concentrated sulphuric acid mixed with sulphate

\* See preceding article, page 135.

of ammonia; and elevated it to a temperature of from  $150^{\circ}$  to  $200^{\circ}$ . The deutoxide of nitrogen was decomposed as in the preceding case, and pure nitrogen was liberated. This gas was mixed with deutoxide of nitrogen, at those times only when the disengagement was too rapidly carried on.

The decomposition of ammonia by the deutoxide of nitrogen in presence of concentrated sulphuric acid, is so easy, the nitrogen produced so pure, and so uniformly liberated from the mixture, that I have no doubt of this re-action being turned to account by chemists, for the preparation of this gas. This new method also possesses great simplicity, because it is sufficient to cause the absorption of the deutoxide of nitrogen, by the sulphuric acid of commerce, and when it is desirable to prepare nitrogen, to take this compound, and add to it sulphate of ammonia, and gently heat the mixture. This re-action is perfectly clear and satisfactory, and leaves nothing more to be desired in the preparation of nitrogen.

As regards the protoxide which forms when the nitrate of ammonia is heated with an excess of concentrated sulphuric acid it is not pure—it invariably contains nitrogen, and is sometimes mixed with traces of reddish bright vapours. It also happens sometimes, that a very small quantity of nitric acid escapes from the mixture, and thus, by its volatility becomes freed from the ulterior action of the ammonia. Nevertheless, the principal re-action, that which evidently regulates all the others, is the transformation of the nitrate of ammonia into protoxide of nitrogen and water.

I have already stated, that nitrate of ammonia heated with ten times its weight of concentrated sulphuric, has yielded nitric acid in such quantity, that only the fourth part of that acid had been destroyed. As I had observed, in this re-action, much of the protoxide of nitrogen, and but a small quantity of the red tinged vapours, I was led to doubt the complete accuracy of a statement that appears in all treatises on chemistry—viz., that concentrated sulphuric acid decomposes nitric acid: 1st. into water, with which it combines; 2nd. into oxygen; and, 3rd. into hyponitric acid. This suspicion became a certainty on observing nitric acid disengaged at  $100^{\circ}$ , from a mixture of nitrate of ammonia with an enormous excess of concentrated sulphuric acid. I have mixed with 500 grammes of concentrated sulphuric acid, 100 parts of nitric acid of the density 1.448. I distilled this mixture slowly, and procured from it 88 grammes of nitric acid of the

density 1.520. This latter acid, when purged of its yellow colour by expelling the red fumes by a gentle heat, was mixed with six and a half times its weight of very concentrated nitric acid, without any perceptible elevation of temperature. This mixture was colourless, and produced extremely dense white fumes of nitric acid. Elevated to a temperature not exceeding  $150^{\circ}$ , and maintained for a long time, as near as possible, to  $100^{\circ}$ , there were distilled over 82 grammes of nitric acid of a density of 1.520, the boiling point being from  $86^{\circ}$  to  $88^{\circ}$ . A third rectification of the sulphuric acid made no change in either the density or the colour of the nitric acid obtained.

I am inclined to think, that the trifling loss observed in repeated distillations of nitric acid with the sulphuric, is much less attributable to the peculiar action of the sulphuric acid, than to that of light, and still more so to the action of heat. This, however, is certain, that the same extent of loss is experienced in the distillation of monohydrated nitric acid when distilled alone, as when distilled with sulphuric acid, and the proportion of red vapours is the same in both cases. The first hydrates of the sulphuric and the nitric acids, appear to me to have no action on one another; there is no elevation of temperature manifested when they are mixed. Nothing proves that one of these hydrates has more affinity than the other, for the water which the latter contains above one equivalent; indeed, concentrated nitric acid, in its turn, can remove water from aqueous sulphuric acid.

The preceding observations have conducted me to the advantageous employment of sulphuric acid for concentrating nitric acid. For the purpose of obtaining the latter acid in every concentrated condition, it only requires that the acid of commerce be rectified two or three times with sulphuric acid of the ordinary quality, with the solitary precaution of not varying the temperature of the mixture from between  $140^{\circ}$  to  $150^{\circ}$ . A gentle ebullition, and, lastly, a small quantity of the brown oxide of lead added to the distilled acid when cold, is sufficient to remove any hyponitric that it may have retained. There does not remain the smallest quantity of lead in the acid thus treated.

The property which ammonia possesses by the operations of its hydrogen, of accomplishing the decomposition of various oxygenous compounds of nitrogen when they are dissolved in sulphuric acid, is susceptible of a very important application in the purification of the sulphuric acid of commerce. That acid is frequently impure by containing deutoxide of nitrogen

and nitric acid, the presence of which is prejudicial in many circumstances. No process is yet known for expelling these nitrous compounds with rapidity and economy from sulphuric acid. The flour of brimstone and lampblack destroy them, it is true, but their employment is subject to some inconveniences which render its abandonment necessary. The sulphate of the protoxide of iron succeeds well; but it is necessary to distil the acid, otherwise a considerable quantity of the sulphate of peroxide of iron would still be left in it. Ammonia, or rather sulphate of ammonia, combines all the requisites that can be desired for the purpose of its rectification. The acids containing the greatest quantity of nitrous compounds, are completely purged of them by one-hundredth part of their weight of sulphate of ammonia; and, in most cases, one or two-thousandths are sufficient. A ready and easy trial leaves not the slightest trace of ammonia in the purified acid, and shows exactly the necessary quantity of sulphate of ammonia to be added to the acid whilst impure. Moreover, should a trace of ammonia happen to remain in the acid, that circumstance would be attended by no inconvenience. At the present price of sulphate of ammonia, the cost of the purification of one hundred kilogrammes of sulphuric acid of commerce would not be more than twelve or fifteen centimes, (a penny or a penny farthing). Besides, there is nothing to be changed in the present process of manufacturing and concentrating this acid. The only thing to be attended to, add to the concentrating acid in the leaden boilers, the two or three-thousandth part of its weight of sulphate of ammonia. This salt dissolves, and the operation proceeds in the usual manner.

The nitrous compounds with which the sulphuric acid of commerce is contaminated, are the cause of the corrosion of platinum vessels when used for concentration. It is to their presence, also, that we must attribute the alteration which indigo suffers in the sulphuric solution of which, are found mixed those yellow matters which are not formed when the acid is pure. It is also said that the clarifying of oils does not proceed so well when nitrous bodies are in the sulphuric acid employed.

Hydrochloric acid prepared by decomposing sea salt with sulphuric acid, necessarily contains chlorine, or nitro-hydrochloric acid, which is attended with many inconveniences, which, amongst others that I now pass by in silence, do not exist when the mode of purification that I have proposed is pursued.

*On the Combustion of Coal and the Generation of Smoke.*

(ROYAL VICTORIA GALLERY, CONVERSAZIONE.)

CHARLES WYE WILLIAMS, Esq., of Liverpool, concluded the series of his communications on the above subject, on Thursday, the 8th July; G. W. BUCK, Esq., in the chair.

The present communication had reference, practically, to the several parts of a furnace, and their respective bearings on the chemical process of combustion, which had been explained on former occasions. With respect to that important part of a furnace, the bridge, some explanatory diagrams were exhibited, outlines of which we here present to our readers. The hollow bridge, for which Mr. Parkes obtained a patent in 1820, was first explained, and the difference between it and the ordinary solid bridge explained. See figs. 1 and 2. Plate IV.

The object sought by Mr. Parkes was to admit the air to the gases at their highest temperature, to effect its combustion. This was done by means of a hollow bridge, the air entering from the ash-pit by an aperture which was furnished with a regulating valve, and issuing at the top where it met the gases passing on their way to the flues. The disadvantage attending this plan was, that the air intended for the gas was too much concentrated into one spot, and therefore, issuing in too large a column or body, thus cooling the gas at the place it was most required to be heated. The primary law, however, as stated by Mr. Williams, was, that no larger body, or number of atoms of air should be introduced by any one aperture than could be absorbed by the atoms of gas with which they immediately came into contact. Although this could not be so well managed when the air entered by so large an orifice, instead of by numerous small jets, still Mr. Parkes' bridge was the best that had hitherto been adopted. Mr. Williams stated that many years ago he had introduced a modification of this bridge, as shown in fig. 3, into several steam vessels, with decided advantage.

The introduction of air by the bridge, in this way, independently of what passed upwards through the bars, necessarily demanded both labour and care from the firemen, lest too much might find its way to the furnace. The result was, that when the fire bars were neglected and left uncovered, too much air was admitted, and the orifice by the bridge, on Parkes' plan, became rather injurious than otherwise. Instead

of attending to their furnaces, and contracting the admission of air by keeping the bars well covered, the firemen attributed the inconvenience they experienced, to the air passing through the bridge; and were in the habit of closing the aperture in the ash-pit by heaping ashes against it. To counteract this, an improvement was adopted, as shown in fig. 4, which embraced the principle of Parkes' bridge, while it prevented the firemen from defeating the intended object, and compelled them to keep their bars better covered. This latter alteration embraces the whole of the plan since adopted by many engineers in Liverpool, and in many steam vessels, and, among others, in the transatlantic steam vessels, the "Liverpool" and the "Oriental," in both of which it remains to this day with the best effect. This mode of constructing the bridge has been found to be the best until Mr. Williams introduced his mode of admitting air by small jets behind the bridge, both in marine and land engines.

Mr. Williams here commented on the error into which the public had been led by the statement, that some smoke-burning experiments had been tried in the steamer, the "Liverpool," and that such experiments had occasioned the great expenditure of fuel, which caused her return to Cork on her first voyage to New York. No experiment whatever had been made: the kind of bridge used was that shown in fig. 4, and which was then the ordinary and most approved plan in the port. Mr. Williams then exhibited a large plan of the boilers of the "Liverpool," and their furnaces; their injudicious construction, and explained their mismanagement during the first voyage, showing that to these was the wasteful expenditure of fuel attributable, and to nothing else.

Mr. Williams then explained the modes now adopted for the arrangements of the flues of marine and land boilers, and exhibited diagrams of the three prevailing systems. In the first, fig. 5., each furnace has its own independent flue; in the second, fig. 6, each furnace led into two separate flues; and, in the third, fig. 7, each pair of furnaces was led into one flue, by which an injurious contraction of space took place exactly at the point where expansion was required. With the first, Mr. Williams observed, that he had succeeded in every instance in which he applied his mode of admitting air behind the bridge. With the second, his success was but partial, and with the third, he had great difficulty. Thus showing the important influence which the arrangement of the flues of a boiler had in effecting a perfect combustion.

Mr. Williams then explained the relation which these several

plans had to the quantity of smoke produced, and concluded by reading the following extract from the forthcoming, second part of his treatise on the combustion of coal.

*On the generation of Smoke.*

A consideration of the nature of those products into which the combustible constituents of coal are converted in passing through the furnace and flues of a boiler, will enable us to correct many of the practical errors of the day, and ascertain the amount of useful effect produced and waste incurred.— These products are—

1st. Steam—highly rarified, invisible, and incombustible.

2nd. Carbonic acid—invisible and incombustible.

3rd. Carbonic oxide—invisible, but combustible.

4th. Smoke—visible, partly combustible, and partly incombustible.

Of these, the first two are the products of perfect combustion, the latter two of imperfect combustion.

The first—steam—is formed from that portion of the hydrogen (one of the constituents of coal gas) which has combined chemically with its equivalent of oxygen from the air—in the proportion of one volume of hydrogen to half a volume of oxygen; or, in weight, as 1 is to 8.

The second—carbonic acid—is formed from that portion of the constituent carbon, which has chemically combined with its equivalent of oxygen, namely, in the proportion of 16 of oxygen to 6 of carbon in weight; or, in bulk, of one volume of the latter, to two of the former.

The third—carbonic oxide—is formed from that portion of the carbonic acid, which, being first formed in the furnace, takes up an additional portion of carbon in its passage through the ignited fuel on the bars, and is then converted from the *acid* into the *oxide* of carbon; thus changing its nature from an incombustible to a combustible. This additional weight of carbon so taken up, being exactly equal to the carbon forming the carbonic acid, necessarily requires for its combustion the same quantity of oxygen as went to the formation of the acid.

The fourth—smoke—is formed from such portions of the hydrogen and carbon of the coal gas as have not been supplied or combined with oxygen, and consequently have not been converted either into steam or carbonic acid.

The hydrogen so passing away is transparent and invisible; not so, however, the carbon, which, on being so separated from



the hydrogen, loses its gaseous character, and returns to its natural and elementary state of a black pulverulent body. As such, it becomes *visible*, and this it is which gives the dark colour to smoke.

Not sufficiently attending to these details, we are apt to give too much importance to the presence of the carbon, and have, hence, fallen into the error of estimating the loss sustained by the depth of the colour which the smoke assumes, without taking any note of the invisible combustibles, hydrogen, carbon, oxide, which accompany it. The blackest smoke is, therefore, by no means a source of the greatest loss; indeed, it may be the reverse, the quantity of invisible combustible matter it contains, being a more correct measure of the loss sustained than could be indicated by more colour.

This will be still more consistent with truth; should any of the gas (carburetted hydrogen) escape undecomposed, or unconsumed, as too often is the case.

In the ordinary acceptation of the term smoke, we understand *all* the products, combustible and incombustible, which pass off by the flue and chimney. When, however, we are considering the subject scientifically, and with a view to a practical remedy against the nuisance or waste it occasions: we must distinguish between the gas as it is generated, and that which is the result of its imperfect combustion. In fact, without precise terms and reasoning, we disqualify ourselves from obtaining correct views, either of the evil or the remedy.

Now, let us look at this gas, which we are desirous of converting to the purposes of heat, under the several aspects in which it may be presented under varying degrees of temperature, or supplies of air.

In the first instance, suppose the full equivalent of air to be supplied in the proper manner to the gas, namely, by small jets, for in this respect the operation is the same as if we were supplying gas to the air, as in the Argand gas lamp; in such case, one-half of the oxygen absorbed goes to form steam by its union with the hydrogen, while the other half forms carbonic acid, by its union with the carbon. Both constituents being thus supplied with their equivalent volumes of the supporter, the process would here be complete, perfect combustion would ensue, and no smoke be formed; the quantity of air employed being ten times the volume of the gas consumed.

Again, suppose that but one-half, or any other quantity less than the saturating equivalent of air were supplied. In

x

such case, the hydrogen, whose affinity for oxygen is so superior to that of carbon, would seize on the greater part of the limited supply ; while the carbon losing its connexion with the hydrogen, and not being supplied with oxygen, would assume its original black solid state, and become true smoke. The quantity of smoke then would be in proportion to the deficiency of air supplied.

But smoke may be caused by an *excess* as well as a deficiency in the supply of air. This will be understood when we consider that there are *two* conditions requisite to effect this chemical union with oxygen, namely, a certain degree of temperature in the gas, as well as a certain quantity of air, for unless the due temperature be maintained, the combustible will not be in a state for chemical action.

Now, let us see how the condition as to *temperature* may be affected by the quantity of air being in excess. If the gas be injudiciously supplied with air, that is, by larger quantities or larger jets than their respective equivalent number of atoms can immediately combine with as they come into contact, a cooling effect is necessarily produced instead of the generation of heat. The result of this would be, that although the quantity of air might be correct, the second condition, the required temperature, would be sacrificed or impaired ; the union with the oxygen of the air would not take place, and smoke would be formed.

Thus we perceive that the *mode* in which the air is introduced, exercises an important influence on the amount of union and combustion effected, the quantity of heat developed, or of smoke produced ; and in examining the mode of administering the air, we shall discover the true causes of perfect or imperfect combustion in the furnace, as we see it in the lamp. This circumstance, then, as regards the manner in which air is introduced to the gas (like the introduction of gas to the air) demands especial notice, as the most important although most neglected feature in the furnace, and in which practical engineers are least instructed by those who have undertaken the task of teaching others.

But let us trace the several stages or circumstances which lead to combustion in the furnace. These are—

1st. Expelling the bituminous constituents from the coal, in the form of gas ; that is, converting them from the solid to the gaseous state. This is effected by their absorption of heat.

2nd. Decomposing this gas, (carburetted hydrogen,) and

resolving it into its constituents, hydrogen and carbon ; thus preparing each for union with its respective quantity of oxygen, according to its own specific law, and measure of affinity. This is effected by a further absorption of heat.

3d. Raising these two combustibles to the temperature respectively required for chemical and electrical action. This also is effected by a still further absorption of heat.

I may here stop to observe, that this is the stage of the process at which light is given out, and which is almost exclusively attributable to the radiation from the ignited and minutely divided carbon, the atoms of which are then at the highest possible temperature, or temperature—as Sir Humphry Davy observes—beyond the white heat of metals. If, however, these elementary atoms of carbon be not supplied with oxygen at this juncture, they are quickly carried away by the current (improperly termed the draught) and their own diminished specific gravity, and soon losing the required degree of temperature, become unfitted for chemical action, and form the black matter of smoke and soot.

4th. Producing atomic contact (technically called diffusion) between the oxygen of the air, and the atoms of hydrogen and carbon, thus liberated from that union which had before constituted them an hydro-carbon gas.

5th. Affecting the chemical union of those bodies, or so many of their elementary atoms as have obtained contact with their respective equivalents of oxygen, and in as rapid succession as such contact may be obtained. This latter process alone is combustion, all the preceding ones being merely preparatory. This is the process in which the respective electricities of the combining elements are exchanged, when heat is developed, and when new and distinct bodies are chemically formed.

We perceive throughout the whole of these several stages, that the combustibles, in their progress towards combustion, are uniformly absorbing heat, the last stage alone being that in which new heat is generated, and which, in its turn, is to impart the required temperature to other atoms as they successively enter on a similar course. We see also that the interruption of this progression at any one stage, involves the escape either of the compound gas or its elementary atoms, and their conversion into smoke.

We see then how palpably erroneous is the idea that smoke, once formed, can be consumed in the furnace in which it is generated, and how irreconcilable is such a result with the

operations of nature. The formation of smoke, in fact, arises out of the failure of some of the processes preparatory to combustion, or the absence of some one of the conditions, which are essential to that consummation from which light and heat are obtained. To expect then that smoke, which is the very result of a deficient supply of heat, or air, or both, can be consumed in the furnace, in which such deficient supply has occurred, is a manifest absurdity, seeing that if such heat and air had been supplied, this smoke would not have existed.\*

*On the Magnetic Dip in the United States.* By ELIAS LOOMIS, Professor of Mathematics and Natural Philosophy in Western Reserve College.†

*Messrs. Editors*—I have read with much interest the remarks by Prof. Locke in the last number of your Journal, and have in consequence been led to review my former magnetic article published in Vol. xxxix, p. 41. I have carefully compared all of Prof. Locke's observations with such of my own as have been made in Ohio and Michigan, both those which are given in my former article, and those which I have since made. I have followed the method adopted by Major Sabine in his magnetic survey of Scotland. The first column in the following table gives the stations of observation; the second and third give their latitudes and longitudes, taken from Mitchell's large map of the United States, with the exception of places not shown on that map. The longitudes have all been diminished seven minutes, which by my observations is the error in the assigned longitude of Hudson. Column fourth exhibits the observed dip, reduced to Jan. 1, 1840, by assuming the annual motion to be  $-1'.8$ . Adopting for the central position lat.  $41^{\circ} 22'$ , lon.  $84^{\circ} 54'$ , we obtain the differences of latitude and longitude which furnish us the annexed equations of condition.

\* We are promised a series of short essays on this very important subject, and shall feel much pleasure in placing them before our readers, whenever Mr. Williams has an opportunity of favouring us with them.—EDIT.

† Silliman's American Journal.

Stations.	Latitude.	Longitude.	Dip Jan. 1st, 1840.	Equations of Conditions.	Dif. of ob- served and comp'd dip
Louisville .....	38 18 N.	85 37 W.	70 1.7	$1.028 = d - 184x - 33.7y$	+ 17.1
St. Louis .....	38 37	90 11	69 30.8	$.513 = d - 165x - 247.7y$	+ 2.9
Cincinnati .....	39 6	84 23	70 34.2	$1.580 = d - 136x + 24.1y$	- 1.8
Dayton .....	39 46	84 5	71 19.6	$2.327 = d - 96x + 37.7y$	+ 5.0
Springfield .....	39 55	83 42	71 24.3	$2.405 = d - 87x + 55.2y$	- 1.0
Columbus .....	39 57	82 58	71 1.8	$2.030 = d - 85x + 88.9y$	- 30.6
Urbanna .....	40 5	83 39	71 35.8	$2.597 = d - 77x + 57.4y$	+ 1.2
Tallmadge .....	41 6	81 26	72 50.6	$3.843 = d - 16x + 156.7y$	+ 5.7
Windham .....	41 15	81 3	73 4.6	$4.077 = d - 7x + 173.7y$	+ 9.0
Shalersville .....	41 15	81 13	72 57.8	$3.963 = d - 7x + 166.2y$	+ 3.4
Streetsboro' .....	41 15	81 20	72 54.1	$3.902 = d - 7x + 160.9y$	+ .5
Hudson .....	41 15	81 26	72 50.8	$3.847 = d - 7x + 156.4y$	- 2.0
Warren .....	41 16	80 49	73 1.9	$4.032 = d - 6x + 184.2y$	+ 3.8
Hartford .....	41 20	80 34	73 1.0	$4.017 = d - 2x + 195.2y$	- 2.4
Bazetta .....	41 20	80 45	73 0.9	$4.015 = d - 2x + 187.0y$	- 1.2
Aurora .....	41 20	81 20	72 56.7	$3.945 = d - 2x + 160.7y$	- 1.3
Twinsburgh .....	41 20	81 26	72 52.5	$3.875 = d - 2x + 156.2y$	- 4.8
Bedford .....	41 24	81 32	72 59.3	$3.988 = d + 2x + 151.5y$	- .8
Kinsman .....	41 28	80 34	73 9.3	$4.155 = d + 6x + 194.8y$	- 1.2
Davenport .....	41 28	90 35	71 52.9	$2.882 = d + 6x - 255.5y$	- 6.7
Sandusky .....	41 29	82 40	72 56.6	$3.943 = d + 7x + 100.4y$	0.0
Cleveland .....	41 30	81 42	73 19.0	$4.317 = d + 8x + 143.8y$	+ 14.7
Maumee .....	41 34	83 32	72 47.9	$3.798 = d + 12x + 61.4y$	- 7.0
Lost Grove .....	41 39	90 9	72 1.9	$3.032 = d + 17x - 235.4y$	- 10.7
Toledo .....	41 41	83 25	73 4.9	$4.082 = d + 19x + 66.5y$	+ 2.9
Wapsipinnicon .....	41 45	90 23	72 14.4	$3.240 = d + 23x - 245.4y$	- 2.0
Monroe .....	41 55	83 20	73 31.1	$4.518 = d + 33x + 69.9y$	+ 16.1
Brown Settlement .....	42 4	91 2	72 20.9	$3.348 = d + 42x - 273.2y$	- 8.1
Farmer's Creek .....	42 13	90 23	72 32.7	$3.545 = d + 51x - 243.6y$	- 9.0
Ypsilanti .....	42 14	83 32	73 16.8	$4.280 = d + 52x + 60.7y$	- 13.8
Mahogueta .....	42 14	90 57	72 43.1	$3.718 = d + 52x - 268.8y$	+ 4.4
Ann Arbor .....	42 18	83 37	73 12.7	$4.212 = d + 56x + 56.9y$	- 20.8
Detroit .....	42 19	82 56	73 41.4	$4.690 = d + 57x + 87.2y$	+ 2.2
Dubuque .....	42 29	90 26	73 4.6	$4.077 = d + 67x - 244.8y$	+ 8.7
Mineral Point .....	42 52	89 58	73 20.3	$4.338 = d + 90x - 222.8y$	+ .4
Blue Mounds .....	43 1	89 27	73 40.6	$4.677 = d + 99x - 199.6y$	+ 9.1
Prairie du Chien .....	43 4	91 0	73 16.3	$4.272 = d + 102x - 267.4y$	- 7.3
Madison .....	43 5	89 6	74 3.2	$5.053 = d + 103x - 184.0y$	+ 25.6

The preceding equations furnish us, by the method of minimum squares, with the following values:  $d=3.5747$ ,  $x=+.01491$ ,  $y=+.00262$ , and the direction of the isoclinal line is from N.  $80^{\circ} 1'$  W. to S.  $80^{\circ} 1'$  E. Computing from these data the dip at the several stations, we obtain the differences given in the last column above. When the observed dip is greater than the computed, the sign + is prefixed. Eight of these differences are greater than  $10'$ , four of them belonging to Prof. Locke's observations and the others to mine. They are as follows:

## PROF. LOCKE'S.

Columbus	-30.6	-23
Madison	+25.6	+27
Louisville	+17.1	+12
Lost Grove	-10.7	*

## PROF. LOOMIS'S.

Ann Arbor	-20.8	-19
Monroe	+16.1	+13
Cleveland	+14.7	+16
Ypsilanti	-13.8	-15

The numbers in the second columns are the differences given in my former article. The correspondence is certainly as good as could have been expected, considering that the last result is obtained by a comparison of nearly double the number of observations, and by a rigorous computation, while the other results were measured upon a map. At Prairie du Chien the discordance is more considerable. The difference I now find is  $-7'.3$ ; in my former paper  $-20'$ . The discordance is owing in part to the curvature I ascribed to the isoclinal lines, by which most of the observations seemed best represented, though the apparent error of this observation was increased. The differences for the remaining thirty observations are quite moderate, and show that the hypothesis of parallel straight and equidistant isoclinal lines, is not very much in error.

Let us now compare Prof. Locke's observations in the neighbourhood of the Mississippi river with themselves, and see how they accord. The following table is arranged like the preceding, the latitudes and longitudes being as furnished by Prof. Locke. The central position adopted is lat.  $42^{\circ} 0' N.$ , lon.  $90^{\circ} 10' W.$

Stations.	Latitude.	Longitude.	Dip Jan. 1st, 1840	Equations of Conditions.	Dif. of ob- served and comp'd dip
St. Louis .....	$38^{\circ} 36' N.$	$89^{\circ} 36' W.$	$69^{\circ} 31.4$	$.523 = d - 204x + 26.6y$	+ 3.4
Davenport .....	$41^{\circ} 30'$	$90^{\circ} 18'$	$71^{\circ} 53.4$	$2.890 = d - 30x - 6.0y$	- 7.1
Lost Grove ....	$41^{\circ} 39'$	$90^{\circ} 9'$	$72^{\circ} 2.4$	$3.040 = d - 21x + .7y$	- 9.7
Wapsipinnicon ..	$41^{\circ} 45'$	$90^{\circ} 23'$	$72^{\circ} 14.9$	$3.248 = d - 15x - 9.7y$	+ 1.7
Brown Settlement	$42^{\circ} 4'$	$91^{\circ} 2'$	$72^{\circ} 21.4$	$3.357 = d + 4x - 38.6y$	+ 2.9
Farmer's Creek..	$42^{\circ} 13'$	$90^{\circ} 23'$	$72^{\circ} 33.1$	$3.551 = d + 13x - 9.6y$	- 7.0
Mahogueta ....	$42^{\circ} 14'$	$90^{\circ} 57'$	$72^{\circ} 43.6$	$3.727 = d + 14x - 34.8y$	+ 13.8
Dubuque .....	$42^{\circ} 29'$	$89^{\circ} 56'$	$73^{\circ} 5.$	$4.083 = d + 29x + 10.3y$	+ .6
Mineral Point ...	$42^{\circ} 50'$	$89^{\circ} 54'$	$73^{\circ} 20.6$	$4.343 = d + 50x + 11.7y$	- 4.6
Blue Mounds....	$43^{\circ} 1'$	$89^{\circ} 38'$	$73^{\circ} 40.9$	$4.681 = d + 61x + 23.4y$	- .1
Prairie du Chien.	$43^{\circ} 3'$	$90^{\circ} 52'$	$73^{\circ} 16.6$	$4.277 = d + 63x - 30.7y$	- 2.1
Madison .....	$43^{\circ} 5'$	$89^{\circ} 6'$	$74^{\circ} 3.5$	$5.058 = d + 65x + 46.7y$	+ 8.3

These equations being solved in the usual manner, give  $x=.01600$ ,  $y=.00745$ ,  $d=3.5323$ , and the direction of the isoclinal lines is from N.  $65^{\circ} 1' W.$  to S.  $65^{\circ} 1' E.$  Computing the dip from these data we obtain the differences in the last column above. These differences are much less than

those before found, and it seems highly probable that in this vicinity the isoclinal lines make a greater angle with the parallels of latitude than they do in Ohio. Yet the above observations are all embraced within less than two degrees of longitude, and are therefore insufficient to determine with much precision the dependence of the dip upon the longitude. I think it improbable that the inclination should be as great as  $24^{\circ} 59'$  according to these observations; yet admitting such to be the case, we still obtain considerable differences between the observed and computed dip. Are these differences to be regarded as errors of observation, or as errors of the hypothesis of parallel, straight and equidistant isoclinal lines? In order to answer this question, it is necessary to consider all the possible sources of error in magnetic observations.

The errors arising from the inclination of the magnetic axis of the needle to the axis of figure, and from inequality in the weight of the arms, as well as the zero error of the graduation, appear to have been provided against in Prof. Locke's mode of observation. That arising from the eccentricity of the axis of the needle in relation to the vertical circle on which the readings are made, is not alluded to. This error in my instrument commonly amounts to one or two minutes, and sometimes even to five or more. It is corrected by readings at both extremities of the needle. Prof. Locke makes no mention of having employed this precaution, and his language on page 321, where he says "the dip is determined by eight distinct readings of each needle," would seem to imply that he did not attend to it. With a good instrument, no great error would ordinarily arise from this source, yet it might easily amount to  $2'$  or  $3'$ .

A more considerable source of error is that arising from the uncertainty of the readings themselves. A dipping needle will seldom come to rest twice in the same position. This arises, not from a change in the direction of the magnetic force, but from friction on the axis of the needle. Prof. Locke's observations exhibit this fact in a striking light. The difference of the readings with the face of the instrument east and west, and in the same position of the needle, is equal to twice the zero error. Now as this error may be assumed to be constant, we obtain by a comparison of the observations eighty-eight values of the same element, the accordance of which with each other will enable us to judge of the confidence which may be placed in a single observation. It will be seen that the dip is usually the greatest when the face of

the instrument is east. Subtract then the dip observed with the face west from that found with the face east with the same position of the needle. For example, in the first observation  $72^\circ 47'$  from  $72^\circ 5'$  gives  $-42'$ , and so of the rest. We thus obtain the following table, which exhibits the observed values of twice the zero error.

-42	+14	+11	-1	+7	+2	+1	+20	+11.5	+4	+8
+19	-3	+3	+3	+8	+13	-2	+15	+7	+7	+12
+6	+7	-2	+5	+7	-4	-7	+9	+5.5	-3.5	+19
+5	-10	+4	+7	+10	+1	-12	+9	+13.5	-13	+16.5
+4	+4	-2	+5	0	+3	-1	+4	+1	+10	+8
+1	-1	+3	+9	+17	+5	0	+3	+9	+5.5	+3.5
+3.5	+11	+6	+3	+5	+8	-4	+12	+8	+10	+6
+2.5	+8	+10	+10	+1	+10	-4	-3.5	+6.5	+2.5	+6.5

The mean of all these observations is  $+4'.5$ , which may be taken as equal to twice the zero error. The difference between this and the preceding observations will show the *errors* of the observations, which when classified are as follows:

+15.5	+8.5	+5.5	+3.5	+2.5	+1.5	-0.5	-1.5	-3.5	-5.5	-8.5
14.5	7.5	5.5	3.5	2.5	1	.5	1.5	3.5	5.5	8.5
14.5	7.5	5.5	3.5	2.5	1	.5	1.5	3.5	6.5	8.5
12.5	7	5.5	3.5	2.5	.5	.5	1.5	3.5	6.5	11.5
12	6.5	4.5	3.5	2	.5	.5	1.5	3.5	6.5	14.5
10.5	6.5	4.5	3.5	2	.5	1.0	2.	4.5	7.5	16.5
9.5	5.5	4.5	2.5	1.5	.5	1.0	2.	4.5	8	17.5
9	5.5	4.5	2.5	1.5	.5	1.5	2.5	5.5	8	46.5

By far the greatest error here is  $-46'.5$ , which was obtained from the first observation. The difference between the readings with the face of the instrument and needle both east, and that with the former west and the latter east, instead of being  $-42'$ , should be  $+4'.5$ . This does not inform us which reading is most in error; if, however, we apply the correction to  $72^\circ 5'$ , making it read  $72^\circ 51'.5$ , the resulting dip would be  $71^\circ 54'.1$ , corresponding nearly with the other observations at the same place. The average of all the preceding errors is  $\pm 5'.5$ , which may be taken as the probable error of a single reading entirely independent of instrumental errors, and the error frequently amounts to about a quarter of a degree. What then are we to understand by this result? Simply this, that if the instrument be properly adjusted, and a number of different readings be made *in the same position of the instrument and needle*, the needle each time being raised



from its supports and allowed to come to a state of rest, the readings will not be identical. They will frequently differ  $\pm 15'$  from the mean, and on the average  $\pm 5'.5$ . This is the conclusion derivable from Prof. Locke's observations, and the result I presume coincides substantially with the experience of all who have undertaken similar observations. My attention has been particularly directed to this very annoying and almost disheartening anomaly, and it has appeared to me that when the agate supports and the axis of the needle are carefully wiped clean of moisture and dust, the discordance of the readings arises mainly from the needle's slipping upon the agates to the east or west; and that when the *y's* which elevate the needle are so disposed as to allow the least possible motion in that direction, the accordance of the readings is the best.

This uncertainty in the readings is of itself sufficient to entitle the dipping needle to the character of 'one very ungrateful instrument.' Most of the other errors may be corrected by suitable precautions and reversals; but this cannot be thus annihilated, and the only remedy with which I am acquainted is to multiply observations. I am accustomed to make five observations in each position of the needle and instrument, always reading at both poles. I thus obtain eighty readings with each needle.

Another error, and one which equally affects both needles, arises from observations being made *out of* the meridian. At Hudson, the dip increases less than one minute from being observed two degrees out of the magnetic meridian. Where one has leisure therefore to determine the magnetic meridian with accuracy, this error may be pronounced insensible; but on a tour where observations are usually hurried, the error from this source may become important.

Another source of error is found in the imperfection of the axes of the needles. It has long been known that different instruments would give different values of the dip at the same time and place. This fact is strikingly exhibited in the observations by Captain Ross, contained in the fifth report of the British Association. The dip at London, as given by eight different needles, was as follows:

69° 1'.5	69° 18'.9
6'.3	19'.6
11'.3	21'.8
16'.1	42'.6

Here we have a difference of 41' in the results of two of the needles. This discordance was satisfactorily traced to the

Y

imperfection of the axles, and its effect may be in a measure corrected by making the axle turn in the needle, thus enabling the points of the circumference of the axle in contact with the supporting planes to be varied in successive trials; or it may be corrected by observations in different azimuths. The dip may be deduced from the angles of inclination observed in any two azimuths  $90^\circ$  apart from each other, by the formula  $\cot.d = \cot.i + \cot.\psi$ ; or it may be derived from the formula  $\cot.d = \cot.i \sec. \theta$ . Without some such trial or comparison with a standard instrument, no needle can be certainly relied upon. I have made this trial with my instrument, observing in every  $10^\circ$  of azimuth in the usual manner. Thus one thousand three hundred and sixty readings were made with each needle. I have made in all about four thousand readings to determine the magnetic dip at Hudson, and after all should not dare to use any stronger language than Prof. Locke employs respecting his own results derived from sixteen readings, that they "are accurate within at least two or three minutes of a degree."

Other errors arise from the presence of magnetism, as for example, in the instrument itself, iron about the person of the observer, which may sometimes inadvertently happen with the most cautious, loose iron lying unperceived in the vicinity, etc.; and finally, local attraction sometimes causes the dip at a given place to differ from that due to the geographical position by several degrees. This will be especially noticeable in the vicinity of iron mines, basaltic rocks, etc.

From the preceding remarks, I think it will be seen, that in magnetic observations we are not to look for the precision of astronomy. We have not sufficient data for estimating the *probable* error of one of Prof. Locke's results; yet I should not hesitate to admit a *possible* error of more than  $10'$  independent of local attraction, and this cause might easily increase the error to half a degree. I do not see how Prof. Locke can refuse his assent to this, after publishing the dip at Cincinnati to be in Nov. 1837,  $70^\circ 45'.7$ , and in April, 1840, writes, "I have lately found the dip at Cincinnati to vary between  $70^\circ 25'$  and  $70^\circ 29'$ ," and yet in his last article he assigns  $0'.86$  as the *limit* of instrumental error. As for the errors of my own observations, given on page 87, I have twice observed the dip at Cleveland, on two opposite sides of the city, and in both instances have obtained a result greater than was to have been expected from its geographical position. The other three observations were in Michigan, where I was told iron ore was quite abundant.

*An account of Earthquakes in different Centuries, and at different periods of the year.\**

M. ALEXIS PERREY, Fellow of the Faculty of Sciences at Dijon, announces in a letter to M. Arago, that he is occupied in making historical researches on earthquakes. To the letter is added a specimen of this work, which the author gives with the view of exciting remarks on the plan which he has followed, and which he is willing to modify if necessary. M. Perrey expresses his regret at not having had at his disposal several large chronological collections, in which he would probably have found registered a good number of facts relating to the subject on which he is engaged. However, the sources which he has hitherto been able to consult, have given during thirteen centuries (from 306 to 1583) the number of 262 earthquakes, which, spread some by months, and some over seasons, seem already to enable us to foresee the existence of an inequality in the degree of frequency of this class of phenomena at different periods of the year. We adjoin the table which M. Perrey has prepared.

METEOROLOGY.—Summary, by Months and by Centuries, of Earthquakes, from the year 306 to 1583.

CENTURIES.	WITH DATES OF DAYS OR MONTHS.												WITH DATE OF SEASONS ONLY.		Without any date except that of the year.	Total in the Century.
													Autumn and Winter.	Spring and Summer		
	Jan.	Feb.	Mar.	Apr.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.				
4th....	1	..	1	1	..	..	1	1	..	1	..	1	2	..	8	17
5th....	..	..	..	2	..	2	1	..	3	..	1	..	1	..	6	16
6th....	1	1	..	2	2	1	3	2	1	1	2	4	1	..	6	27
7th....	..	..	..	..	..	1	..	1	..	..	..	..	..	..	3	5
8th....	3	3	..	..	..	..	..	..	..	2	1	1	1	..	8	19
9th....	5	1	..	1	..	..	..	2	2	2	..	7	4	2	4	30
10th....	..	..	..	1	..	..	..	2	1	..	..	..	..	..	4	8
11th....	1	1	6	1	2	..	2	3	2	4	2	..	1	..	5	30
12th....	4	3	..	3	2	3	..	2	2	1	..	4	3	..	7	34
13th....	1	2	1	..	3	1	1	..	2	1	1	3	..	..	8	24
14th....	3	..	1	..	1	3	..	..	1	1	..	1	1	..	3	15
15th....	..	..	..	..	1	..	1	2	..	1	1	3	..	..	5	14
16th....	2	1	1	3	1	2	..	1	2	1	1	4	..	..	4	23
Total by Months	21	12	10	14	12	13	9	14	15	17	10	28	14	2	71	262
	Wint. 43			Spr. 39			Sum. 38			Aut. 55						

\* From the French Academy of Sciences.

The two months of January and December furnish for the winter solstice.....	49
June and July for the summer solstice .....	22
March and April for the vernal equinox .....	24
September and October for the autumnal equinox .....	32
The six months from October to March, autumn and winter, present .....	112
From April to September, spring and summer .....	79
	191

*Experimental Researches on the Production of Silicon from Paracyanogen.* By SAMUEL BROWN, M.D., Edinburgh.\*

IN a former paper, this distinguished chemist intimated that he had been led to infer from experiment, that two familiar substances, long universally regarded as distinct elements, are in reality only modifications of the same material form; and having extended his inquiries, he now maintains that carbon and silicon are isomeric substances. The present communication is of a purely practical character, the author having refrained from presenting what he conceives to be the rationale of the singular facts he has just discovered, until farther investigations of a similar kind shall have been executed by himself and others. The manner in which the author establishes the isomerism of carbon and silicon is very simple, and consists in giving a great many processes by which the former may be converted into the latter. These are contained in a series of five sections: the first treats of the production of silicon from free paracyanogen (N, C, C + N; C, C); the second, on the formation of mixed siliciurets of copper, iron, and platinum, by the re-action of paracyanogen; the third of the quantity of nitrogen separated from paracyanogen, when it is changed into nitrogen and silicon; the fourth describes processes for the preparation of transparent crystallized siliciurets of iron from the paracyanide of iron, and the ferrocyanide of potassium; and the fifth gives easy processes for the extraction of silicon from the ferrocyanide of potassium by the action of carbonate of potassa. It is impossible to convey an adequate conception of the manner in which those subjects are handled in the respective sections, in the form of an abstract. Suffice it that the author, having procured silicon, siliciurets, and silicic acid, in circumstances which appear to

\* London Monthly Journal of Medical Science.—June, 1841.

warrant the conclusion that the carbon of the compounds employed had been transformed into silicon, his chief care was bestowed on the attempt to discover some source of fallacy in his processes, *but in vain*. For example, one of the processes given in the last section is this:—Mix equal weights of anhydrous ferrocyanide of potassium and carbonate of potassa, and keep the mixture five hours (for 2000 grs. of the ferrocyanide) at a good heat, in a well closed crucible of hammered iron; the product yields to water a mixed solution of cyanide of potassium, undecomposed carbonate of potassa, and silicate of potassa. The silicic acid may be separated by adding excess of hydrochloric acid, desiccation, ignition, and elutriation. But we must refer to the paper itself, about to be printed in the Transactions, for numerical and other details. It may be added, however, that the proof that the silicon of the silicic acid procured in the process which has just been mentioned, is not extracted from the iron crucible is *twofold*; first, carbonate of potassa may be employed *alone* without producing any silicate, and that in the same crucible before and after the operation has been performed with the ferrocyanide of potassium; and secondly, the process may be repeated many times in the same crucible, and yet be invariably attended with the same result.

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*Execution and Resuscitation of a Murderer.—Wonderful effects of Galvanism.\**

JOHN WHITE, convicted of the murder of Messrs. Gwatkin and Glenn, on board a flat boat on the Ohio river, was executed at Louisville, United States, on the 8th ult., a little after six o'clock in the morning. The rope not "playing" well occasioned the knot to slip up over the chin instead of being under his ear, so that his neck was not broken by the fall. Previously to his execution he wrote a letter to his father, in which he stated that he was present when the unfortunate men were murdered; that he did not participate in the act, but was compelled to beg his own life from two men who murdered them. He was cut down after hanging about twenty-five minutes, and his body given to the doctors for the purposes of experiment. The *Louisville City Gazette* gives the annexed extraordinary circumstances attending an experi-

\* From the *Louisville City Gazette*.

ment with the galvanic battery :—" The poles of a powerful galvanic pile, which had been prepared for the occasion, were immediately applied to him, and, to the unutterable joy of all present, with the most perfect success. On the first application of the fluid to his body, which was yet warm and trembling, a universal tremor was seen to pass over his frame : on a sudden he arose from his bench to a sitting posture, and with great eagerness and impatience raised his hand to his neck, trying to grasp the scarf in his fingers and tear it from his throat ! He first snatched at it with great rashness, as though the rope was yet around his neck, and then continued some moments picking at the seam with his fingers, as though it was something that adhered to his throat giving him great uneasiness. But this symptom was soon forgotten, for almost the next moment he rose upon his feet, raised his arms level with his breast, and opening his bloodshot eyes, gave forth from his mouth a most terrific screech, after which his chest worked as if in respiration, in a very violent manner. Every one at this minute was as mute as death, when one of the surgeons exclaimed that he was alive. The excitement was too great to allow time for a reply to the remark ; every eye was riveted upon the agitated and shaking corpse. The operator continued to let upon it a full quantum of the galvanic fluid, till the action upon its nerves became so powerful that it made a tremendous bound, leaping by a sort of imperfect plunge into a corner of the room, disengaging itself entirely from the wires which communicated the galvanism. All immediately drew around the body. For a moment after its fall it seemed perfectly motionless and dead, a surgeon approached, and taking hold of his arm, announced that he thought he felt a slight though a single beat of the pulse. The galvanic operator was just going to arrange his machine to give him another charge, when the surgeon exclaimed that he breathed. At this moment he gave a long gasp, rising and gently waving his right hand ; his sighs continued for two minutes, when they ceased entirely. His whole frame seemed to be agitated, his chest heaved, and his legs trembled. These effects were supposed to be caused by the powerful influence of the galvanic fluid upon the nerves ; none of these movements were yet supposed attributable to the action of life. It was considered that the animating principle of nature had left his frame and could never be again restored. In the very height of anxiety, the surgeon announced that he could feel feeble pulsations. A piece of broken looking-glass was immediately held before his nostrils, which was instantly covered with a

cloud. The most intense anxiety was felt for some seconds, when the motion of his chest, as in the act of respiration, became visible. He rolled his eyes wildly in their sockets, occasionally closing them, and giving most terrible scowls. In about five minutes his breathing became tolerably frequent—probably he would give one breath where a healthy man would give four. His breathing, however, rapidly increased. The doctors began to speak to him, but he gave no indications that he heard a word. He looked upon the scene around him with the most deathlike indifference. A young medical student approached him, and, taking hold of his arm and shoulder, White rose upon his feet, took two steps thus supported, and seated himself in an arm chair. His muscles seemed to relax, and he appeared somewhat overcome with the exertion he had made. A bottle of hartshorn was immediately applied to his nose, which revived him, but his life seemed to be that of a man much intoxicated. He seemed upon one occasion to try to give utterance to some feeling, but, from an unknown cause, an impediment probably occasioned by the execution, he was unable to give utterance to a word. His system was critically examined, and, though he was pronounced by the doctors to be perfectly alive, yet he could live but a very few minutes, for congestion of the brain was rapidly taking place. Every method was adopted to equalize the circulation and save the patient from the terrible consequence of so sad a catastrophe, but in vain. The blood vessels of the head were enormously distended, and his eyes appeared to be balls of clotted blood. His system was immediately thrown into direful spasms, and he died in a few minutes in the most excruciating agonies.

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## NOTICES OF NEW BOOKS.

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*Seventh Annual Report of the Royal Cornwall Polytechnic Society.*—Simpkin and Marshall, London.

THE progressively increasing importance of the Royal Cornwall Polytechnic Society is a splendid example of the universal utility that would accrue to science and commerce, in every province where similar institutions can be formed, and supported with the same spirit of liberality, and governed in

the same judicious manner. Notwithstanding, however, the "seven years of increasing usefulness and prosperity, during which this society has been in active operation,"—placing its labours annually before the public in a handsome and well written volume, and thus, year after year, showing to every province of the land the extensive amplitude of usefulness that would be derived by every class in the community were the laudable example, thus manifested, properly followed out in those large towns and rich districts which, in many points of view, are so highly favoured for purposes of such a high import, yet it is lamentable in the extreme to have to acknowledge that in this kingdom the Royal Cornwall Polytechnic Society remains to this day a solitary instance of institutions of this kind.

By the Seventh Annual Report we find that this Society awarded upwards of three-score prizes in medals and money, for various productions of science and art, and that many prizes are announced for future discoveries and inventions.

The Report, properly so called, drawn up by the Rev. J. Bennet, is a very elegant documental outline of the principal topics connected with the interests of the Society which had occurred since the publication of the Sixth Annual Report. The account of the Exhibition is prefaced by several eloquent addresses to the assembly, by various gentlemen of high scientific rank, amongst whom we may notice the Rev. Dr. Buckland, whose address was full of well chosen topics, and apposite remarks.

Mr. Courtney has given an interesting paper of thirty-six pages, consisting of "Chronological Memoranda relating to the town of Penzance," which is followed by an account of the "Total Amount of the private and public Capital and Income of the United Kingdom of Great Britain and Ireland; on the National Debt, and on some of the measures that have been practised or recommended for diminishing it." By Mr. John Allen. After this appears a most excellent essay, "On the Atmosphere of Cornish Mines," by Mr. M. P. Moyle. The remaining articles in this volume are headed as follows:—

Arthur and Eddy's Apparatus for raising Mine Pumps; Phillips's Underground Dial; Tregaskis's Moveable Triangular Sluice Boxes; Tregaskis's Machine for Discharging Ships; Hoskings's Compound Valve for Large Pumps; Wilton's Graduating Machine; Phillips's Trigonometer, Phillips's Proportioner; Remarks on the Efficiency of the Steam Engine, and the methods of measuring it, by the Rev.



H. Moseley, M. A., &c., &c., Professor of Nat. Phil., King's College, London; on the Comparative Longevity of our Mining Population, by Mr. Robert Blee, jun. On the preparation of the White Oxide of Arsenic (Arsenious Acid) in Cornwall," by W. J. Henwood, Esq., F. G. S., London and Paris, Secretary of the Royal Geo. Society of Cornwall, &c., &c.; a description of a new arrangement of the Helio-graph, for Registering the Intensity of Solar Light, by Mr. T. B. Jordan, Mathematical and Philosophical Instrument Maker, and Secretary to the Society. The volume is very neatly got up, and accompanied by four copperplates. These Annual Reports invariably contain a number of very interesting articles.

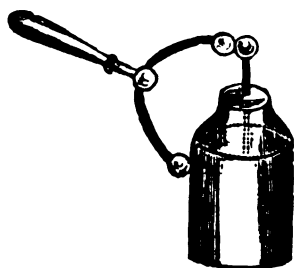
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## ELEMENTARY LECTURES ON ELECTRICITY, &c.

### LECTURE X.

THE Discharging Rod usually employed by Electricians consists of a glass handle and two metallic arms: the arms are of stout brass wire, each terminating in a ball of the same metal. The inner ends of the arms are screwed into two circular pieces of brass, (one into each) which move freely on a brass pin that passes through their centres, and thus form a joint similar to the joint of a pair of candle snuffers, or the joint of fire tongs. To the joint is fixed a brass socket, by which the whole of the metallic part of this apparatus is attached to the glass handle. The arms of the discharging rod are usually bent so as to form two similar bows, and are pointed at the outer extremities, but the points are covered with the before-mentioned balls, which screw over them and can be removed at pleasure when required for any particular experiment.

Fig. 11.



Having now described the Leyden jar and discharging rod, we are prepared to pass through a considerable number of experiments with these important pieces of electrical apparatus. Let us now charge the jar by holding its ball in contact with the prime conductor, whilst the machine is in motion. As the jar is but a small one a short contact will be sufficient to accomplish the charge. I now take it away and apply the discharging rod in the manner represented by fig. 11, and you see a brilliant

flash of light, which is attended by a smart report. This process is usually called *discharging the jar*.

In the figure you will observe that one of the balls is placed against the side of the jar ; which, with some persons, is a very common practice, though very frequently attended with consequences fatal to the jar ; especially when made of very thin glass, which becomes broken by the discharge. Hence you will readily understand that some other method of discharging our Leyden jars, especially when large, and consequently expensive, must be a very desirable object. Several methods of discharging the Leyden jar, without the risk of breaking it, have been proposed ; the principal ones of which are the two following. One of these is to place a long intervening wire between the outside of the jar and the lower ball of the discharging rod ; and thus, by lengthening the circuit, the electric force is somewhat diminished and the jar preserved. This plan has been frequently resorted to when the jar is very large ; or when several of them connected in the shape of a battery, of which I shall speak more particularly in another place, but such long circuits invariably lessen the electric force ; a lower charge of the jar through a short circuit would answer quite as well. The other plan, which in my opinion is much preferable to the former, is to place the jar, when charged, on a plate of metal considerably larger than the bottom of the jar, and then place the lower ball of the discharging rod upon the metallic plate and bring the upper one quickly towards the ball of the jar, by this process the fluid which passes through the discharging rod from the inside of the jar arrives at the bottom of the outside in a state of considerable attenuation as compared with the condensed and compact form it assumes when passing from the ball in close contact with the side of the jar, on which the whole force impinges and is exerted on a mere point of the glass, the fracture of which is almost certain, from an intense charge.

Respecting the theory of the Leyden jar, although various opinions have been advanced : that proposed by Dr. Franklin seems to approach nearest to truth. That philosopher supposed that the Leyden jar, has at all times of its existence the same, or nearly the same quantity of the electric fluid on its two surfaces ; that is, whether the glass be neutral or charged, the absolute quantity of fluid is not altered ; and he accounts for it in this manner :— Franklin supposes that when a jar is in that condition called *charged*, the redundancy is only on one side of the glass ; and that, in all cases, a deficiency of fluid, to precisely the same amount, takes place on the opposite side of it. Let us suppose for instance, that one hundred particles of the electric fluid be forced on to the inner surface of the jar, by the power of the machine ; then, according to Franklin's theory, one hundred particles will, or must of necessity, have left the outside surface ; so that the *charging process* would appear to be nothing more than a mere transference of the fluid from one

side of the glass to the other, and we are told by the same philosopher, that the *discharging* process, by the application of the discharging rod, is simply a restoration of the electric equilibrium of the two sides of the glass.

In the ordinary processes of charging and discharging jars, Franklin's theory affords a tolerably plausible explanation; but I shall have to show you in another place, that glass can be similarly electrized on its two opposite surfaces as decidedly as any other body: and therefore that part of the theory which insists on the two surfaces being necessarily in opposite electric states when charged, must fall to the ground.

Franklin, however, has contrived some very beautiful experiments in favour of his theory. He has shown that if the outside of a Leyden jar be completely insulated, its inside surface is incapable of receiving a charge. If, for instance, we unscrew the ball from the top of the wire of the jar, and put a ring in its place, then we can hang the jar on the wire in the end of the prime conductor, and leave its outside completely insulated by the surrounding air. In this state very little accumulation of fluid will take place although every exertion be made with the machine, and you will see, that if we stop the working of it before the application of the discharging rod, scarcely a spark is discoverable between the ball of the latter and the ball of the conductor to which it is applied. But if we vary the experiment by presenting the knuckle close to the coating of the jar whilst the machine is in motion, you will see a series of sparks traversing the plate of air between the jar and the hand, and the jar becomes charged, as may be seen by applying the discharging rod. If now, instead of the knuckle, we were to apply to the outside of the jar a brass ball mounted on a metallic stem, we shall have better defined sparks than before: and by bringing this ball gradually towards the outside of the jar we can ascertain the precise distance between the two at which a spark will pass from one to the other during various stages of the charging process. Now this experiment which has been but little attended to, is exceedingly interesting; for you will see that the plates of air which the sparks are able to traverse are of very different thickness at the different periods at which we obtain them; the maximum thickness being at the commencement, and the minimum thickness at the termination of the charging process, and eventually the sparks cease altogether. At this stage the jar is fully charged; and if we do not apply the discharging rod immediately it will very likely discharge itself, either by the fluid rushing over the top of the jar, or by perforating the glass. Both of these accidents very frequently happen when the discharging rod is not applied in time. In the latter case, the jar is for ever afterwards useless as an electric apparatus; but, the discharging over the top does not injure it, unless indeed, a zigzag streak, which the fluid very frequently leaves on that part of the glass over which it travelled, can be called an injury. When a jar thus discharges

itself the occurrence is called a *spontaneous discharge*: and although a discharge through the pores of the glass is not designated by that name, it is, as decidedly as the other, a *spontaneous discharge*.

Another of Franklin's experiments shows, in a very decisive manner, that the fluid which is forced on the inner surface of the jar does not lodge in the metallic lining. For this experiment we generally employ a jar furnished with metallic coatings which can be removed from the surfaces of the jar, or replaced at pleasure. The jar for this purpose is often a frustum of a cone with the smaller end downwards, for the purpose of giving facility of placing and displacing the metallic parts. This, however, is not necessary, as common cylindrical jars answer very well, by not making the metallic coating and lining to fit the glass too closely. The lining of this jar is furnished with a brass wire stem which reaches a few inches above the top of the jar and terminates in a ring.

To make Franklin's experiment with this jar, I present the ring to the ball of the prime conductor, and when charged I take it away and place it on a quire of dry paper, or a piece of glass; or, indeed, any very inferior conductor. Then by means of a small glass rod which I introduce to the opening of the ring, I remove the metallic lining and place it on the table. I now take hold of the jar by its coating, and invert it on the insulating body; and take away the metallic coating. I now bring the coating into contact with the lining, and you do not perceive any spark pass between them: and now, indeed, we might apply either metallic body or both of them to the most delicate electroscope, without indication of electric action; hence we say they are neutral. I now replace the coating on the outside of the jar, and turn it the right side upwards; and afterwards replace the lining by means of the insulating glass rod. The metallic appendages being thus both in their proper situations, and consequently the apparatus again complete, I apply the discharging rod, first to the coating with the lower ball, and then to the ring with the other, and you see that a bright flash of light, with the usual report, is the consequence. From this result, Franklin has very properly inferred, that the charge is not lodged in the metallic lining, but either in the pores of the glass or in a thin stratum of air on its surface. This inference forms another portion of the Franklinian theory of the Leyden jar.

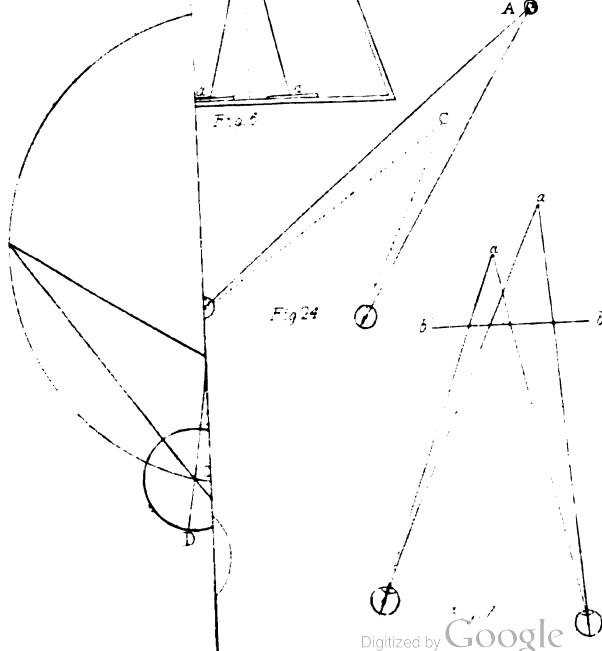
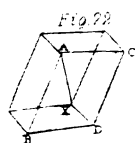
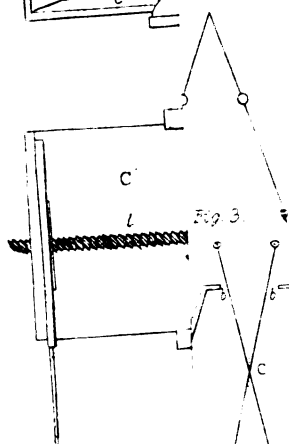
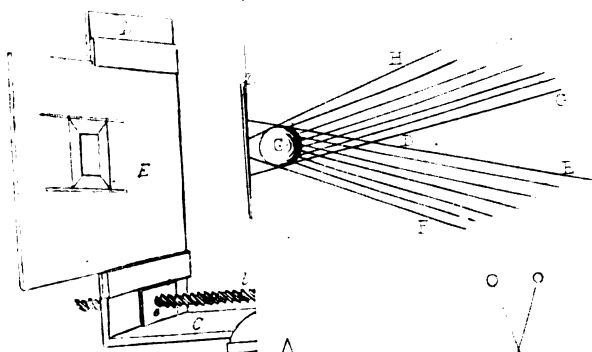




Fig 5

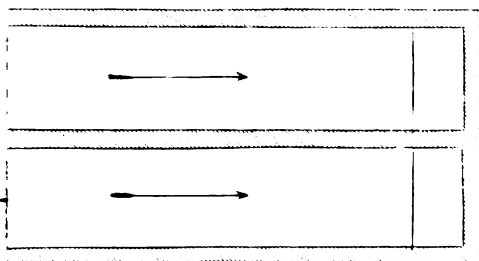


Fig 6

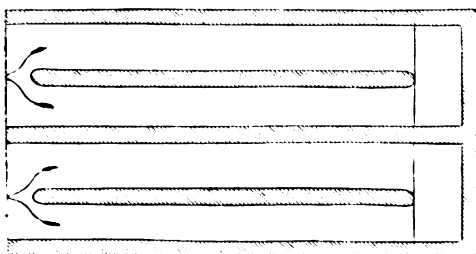
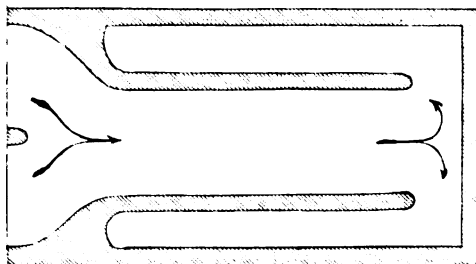


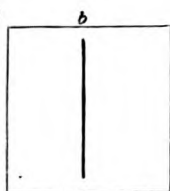
Fig 7







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• a • • fig 11 • • • b •

fig 14

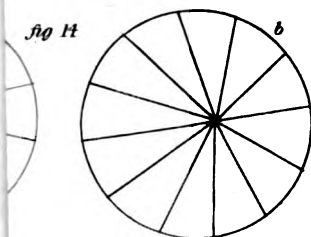


fig 16

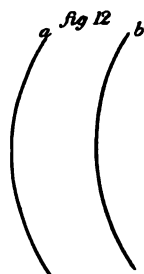
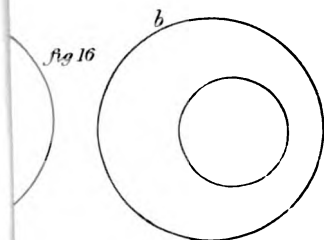


fig 18

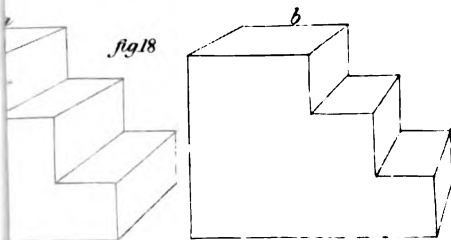
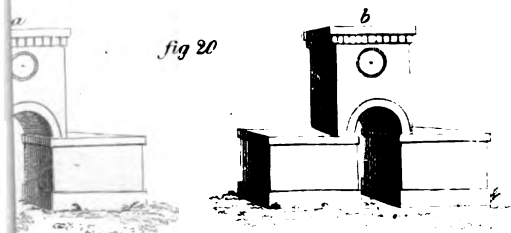


fig 20





THE ANNALS  
OF  
ELECTRICITY, MAGNETISM,  
AND  
CHEMISTRY,  
AND  
**Guardian of Experimental Science.**

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SEPTEMBER, 1841.

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*The Bakerian Lecture. On the Relations of Electrical and Chemical Changes. By Sir HUMPHRY DAVY, Bart. Pres. R. S.\**

I. Introduction.

A long time has elapsed since I read before this Society the Bakerian Lecture on the Chemical Agencies of Electricity. The general laws of decomposition developed in that paper were immediately illustrated by some practical results, which the Society did me the honour to receive in a very favourable manner; and which, by offering a class of new and powerful agents, led me away for many years into a field of pure chemical inquiry: and it is only lately, and on an occasion which is well known, that I have again taken up the subject of the general principles of electro-chemical action. After a number of new experiments, which I shall have the pleasure of laying before the Society, and notwithstanding the various novel views which have been brought forward in this and in other countries, and the great activity and extension of science, it is peculiarly satisfactory to me to find that

\* From the Philosophical Transactions for 1826. Part III.

I have nothing to alter in the fundamental theory laid down in my original communication; and which, after a lapse of twenty years, has continued, as it was in the beginning, the guide and foundation of all my researches.

I am the more inclined to bring forward these new labours at the present moment, though they are far from being in a finished state, because the discovery of Oersted and that of Morichini, illustrated by some late ingenious inquiries, connect the electro-chemical changes with entirely new classes of facts, induce a hope that many of the complicated phenomena of corpuscular changes, now obscure, will ultimately be found to depend upon the same causes, and to be governed by the same laws; and that the simplicity of our scientific arrangements will increase with every advance in the true knowledge of nature.

## II. *Some historical details.*

As I am not acquainted with any work in which full and accurate statements on the origin and progress of electro-chemical science are to be found, and as some very erroneous statements have been published abroad, and repeated in this country, I shall take the liberty of laying before the Society a short historical sketch on this subject; which is the more wanted, as the journal in which the early discoveries were registered has long been discontinued, and is now little known or referred to.

As there are historians of chemistry and astronomy who date the origin of these sciences from antediluvian times, so there are not wanting persons who imagine the origin of electro-chemical science before the discovery of the pile of Volta; and Ritter and Winterl have been quoted\* amongst other persons as having imagined, or anticipated the relation between electrical powers and chemical affinities, before the period of this great invention. But whoever will read with attention Ritter's "Evidence that the galvanic action exists in organized Nature†," and Winterl's "*Prousiones ad Chemiam Sæculi decimi noni*," will find nothing to justify this opinion. Ritter's work contains some very ingenious and original experiments on the formation and powers of single galvanic circles; and Winterl's some bold, though loose spe-

\* Oersted, translated by Marcel, 1813.

† Jena, 1800

culative views\* upon the primary causes of chemical phenomena: and in the obscurity of the language and metaphysics of both these gentlemen, it is difficult to say what may not be found. In the ingenious, though wild views, and often inexact experiments of Ritter, there are more hints which may be considered as applying to electro-magnetism than to electro-chemistry; and Winterl's miraculous *Andronia* might, with as much propriety, be considered as a type of all the chemical substances that have been since discovered, as his view of the antagonist powers, the acid and basic, can be regarded as an anticipation of the electro-chemical theory. The queries of Newton at the end of his "Optics" contain more grand and speculative views that might be brought to bear upon this question than any found in the works of modern electricians†; but it is very unjust to the experimentalists who, by the laborious application of new instruments, have discovered novel facts and analogies, to refer them to any such suppositions as, "that all attractions, chemical‡, electrical, magnetic, and gravitative, may depend upon the same cause; or to still looser expressions, in which different words are used and applied to the same ideas, and in which all the phenomena of nature are supposed to depend on the Dynamic system, or the equilibrium and opposition of antagonist powers.

\* As a specimen of the *Prolusions*, I shall give a few articles from the index which will show the character of the work. *Prolusions*, pag. 256, et seq.

256. "Adamas est Andronia.

260. "Andronia cum Plumbo creat Barytam, Cum Ferro Chalybem.

262. "Carbo est acidus cum Atmosphæra basica.

263. "Chromium non est nisi Calx Magnesii acida.

— "Cuprum cum Androniâ coalescit in Molybdænum.

268 "Scintilla electrica formatur à Principiis Conductorem primum et secundum animantibus, ac inter se concurrentibus; est gravis, habet effectum electricitati contrarium."

† See the eloquent observations of M. Chenevix on the subject of Winterl's Theory, *Annales de Chimie*, vol. 50, 2 cap. 175.

‡ In the *Système Universelle* of M. Azais not only are all the phenomena of nature referred to the same cause, but specific reasonings upon the mode of its operation given. In this work, published in 1810, not only is the identity of magnetism and electricity insisted on, but an attempt is made to explain the manner in which the two electrical fluids produce the magnetic phenomena, pag. 239, vol. i. "Ainsi ces deux ordres de phénomènes sont très ressemblans. Repetons que toutes leurs différences résultent uniquement de ce que les deux fluides sont moins intenses lorsqu'ils produisent les phénomènes du Magnétisme que lorsqu'ils produisent les phénomènes du Galvanisme, &c." It requires only the same principle as that censured in the text to refer to this author the discovery of Oersted and the speculations of Ampère. M. Azais, in his "fluides mineure et majeure," finds all the causes of the acid and alkaline properties of bodies:—slow combinations, the heat produced, and all the phenomena of chemical change; and his reasonings are often very ingenious.

*New Series* Vol. 1. No. 1. Jan. 1827.

The true origin of all that has been done in electro-chemical science was the accidental discovery of M. M. Nicholson and Carlisle, of the decomposition of water by the pile of Volta, April 30, 1800\*. These gentlemen immediately added to this capital fact, the knowledge of the decomposition of certain metallic solutions, and the circumstance of the separation of alkali on the negative plates of the apparatus. Mr. Cruikshank, in pursuing their experiments, added to them many important new results, such as the decompositions of muriates of magnesia, soda and ammonia, by the pile; and that alkaline matter always appeared at the negative, and acid at the positive pole †; and Dr. Henry about the same time made some unsuccessful attempts to decompose potassa in solution by the pile, and confirm the general conclusions of M M. Nicholson, Carlisle, and Cruikshank. In the month of September in this year, I published my first paper on the subject of Galvanic Electricity, in Nicholson's Journal, which was followed by six others ‡: the last of which appeared in January 1801. In these papers I showed that oxygen and hydrogen were evolved from separate portions of water, though vegetable and even animal substances intervened; and conceiving that all decompositions might be polar §, I electrised different compounds at the different extremities, and found that sulphur and metallic substances appeared at the negative pole, and oxygen and azote at the positive pole, though the bodies furnishing them were separate from each other. In the same series of papers I established the intimate connexion between the electrical effects and the chemical changes going on in the pile, and drew the conclusion of the dependence of one upon the other. In 1802 I proved that galvanic combinations might be formed from single metals, or charcoal and different fluids chiefly acid and alkaline, and that the side or pole of the conducting substance in contact with the alkali was positive, and that in contact with the acid, negative; and in the same year I published, that when two separate portions of water, connected by moist bladder or muscular fibre, were electrised, nitro-muriatic acid appeared at the positive, and fixed alkali at the negative pole §. In the same year Dr. Wollaston placed the identity of the cause of galvanism and electricity, which had been always maintained by Volta, out of all doubt, by some very decisive experiments.

In 1804, MM. Hisinger and Berzelius stated that neutro-

\* Nicholson's Journal, vol. xlii. p. 183.

† Ibid. vol. iv. p. 190.

‡ Ibid. pp. 275, 326, 337, 394, 380.

§ Journal of the Royal Institution, 1802. First Series.

saline solutions were decomposed by electricity, and the acid matter separated at the positive, and the alkaline matter at the negative poles; and they asserted that in this way muriate of lime might be decomposed; and drew the conclusion that nascent hydrogen was not, as had been generally believed, the cause of the appearance of metals from metallic solutions. These valuable observations ought to have explained distinctly the source of the appearance of acid and alkaline substances at the two extremities of the pile; yet the paper was never translated into English, nor at all attended to; and one of their facts was contradicted by the assertion of, generally, a very accurate observer, Mr. Cruikshank, who in his early experiments mentioned that he had not been able to decompose muriate of lime in the circuit.

In 1805 various statements were made, both in Italy and England, respecting the generation of muriatic acid and fixed alkali from pure water. The fact was asserted by MM. Pacchioni and Peele, and denied by Dr. Wollaston, M. Biot, and the Galvanic Society at Paris\*. Mr. Sylvester, who conducted his experiments with some care, stated, that if two separate portions of water were electrified out of the contact of substances containing alkaline or acid matter, acid and alkali were generated; so that up to this time the question, whether these substances were liberated from their combinations, or formed from their elements by electricity, could not be considered as decided: a circumstance not so much to be wondered at, when the novel and extraordinary nature of the whole class of galvanic phenomena is considered.

It was in the beginning of 1806† that I attempted the solution of this question; and after some months' labour I presented to the Society the Dissertation, to which I have referred in the beginning of this Lecture. Finding that acid and alkaline substances, even when existing in the most solid combinations, or in the smallest proportion in the hardest bodies, were elicited by voltaic electricity, I establish that

\* Some writers have very incorrectly referred the origin of these researches to the observations of Hisinger and Berzelius; *Annales de Chim.* vol. li. I cap. pag. 167; but these observations were never quoted by any writer of the day on the pretended production of muriatic acid and alkali; and I was not acquainted with them till after my fundamental experiments were finished; and, when in drawing up an account of them, I looked back through the whole series of periodicals publications to find accounts, of experiments bearing upon the same question, and I believe I first directed the public attention to the value of those researches. Whoever will take the trouble to read the Bakerian Lecture for 1806 will be convinced of the gradual development of the whole subject from the investigation respecting the pretended formation of muriatic acid and fixed alkali.

† Phil. Trans. 1807.

they were the results of decomposition, and not of composition or generation; and referring to my experiments of 1800 and 1801 and 1802, and to a number of new facts, which showed that inflammable substances and oxygen, alkalies and acids, and oxidable and noble metals, were in electrical relations of positive and negative, I drew the conclusion, *that the combinations and decompositions by electricity were referrible to the law of electrical attractions and repulsions*, and advanced the hypothesis, "*that chemical and electrical attraction were produced by the same cause, acting in one case on particles, in the other on masses;*" and *that the same property, under different modifications, was the cause of all the phenomena exhibited by different voltaic combinations.*

Believing that our philosophical systems are exceedingly imperfect, I never attach much importance to this hypothesis; but having formed it after a copious induction of facts, and having gained immediately by the application of it a number of practical results, and considering myself as much the author of it as I was of the decomposition of the alkalies, and having developed it in an elementary work, as far as the present state of chemistry seemed to allow, I have never criticized or examined the manner in which different authors have adopted or explained it,—contented, if in the hands of others it assisted the arrangements of chemistry or mineralogy, or became an instrument of discovery. And having now given what I believe to be a faithful sketch of its origin, I shall not enter into an examination of those works which have induced me to make this sketch, and which contain partial or loose statements on the subject, and which refer the origin of electro-chemistry to Germany, Sweden and France, rather than to Italy and England, and which attribute some of the views of the science, which I first developed, to philosophers who have never made any claim of the kind, and who never could have made any, as their works on the subject were published many years after 1806.

### III. *On the modes adopted for detecting the electrical states of bodies, and definitions of terms.*

That the statements made in the following sections may be more distinct, I shall say a few words of the mode in which the different conditions of electrical action were ascertained, and describe the manner in which I have used the terms which have been adopted in electro-chemical science.

In determining the nature of the electrical action in what may be called the close circle, or the combinations in which,



according to the language used on the continent, electrical currents exist, I have employed instruments constructed upon the same principles as the galvanometer of Professor Cumming, or the multiplier of Professor Schweigger. Silver wire covered with silk, about 1-70th of an inch in diameter, was folded round a small wooden frame, so as to fill a narrow deep groove: the two extreme wires were parallel, and the convolutions as nearly as possible in the same perpendicular: a small tube containing a filament of silk was passed through the centre of the convolutions of wires, to which a delicate magnetised needle was suspended; which, when the apparatus was properly disposed, rested with its north pole between the two extremities of the wires. This instrument, which contained 60 circumvolutions of wire, was found sufficiently delicate for most purposes of experiment; but in a few instances, in which very weak electricities were to be determined, I used another apparatus, in which the same kind of wire was fastened, in concentric circles, round two portions of glass tube, in such a manner that radii from the inner circle would have passed through all the wires, and in which increased mobility was given to the system by two needles exterior to it and connected with it, placed one above, the other below the central needle, with their poles in the same directions, but opposite to those of the central needle, which was so magnetised that its directive power was neutralised by the power of the other two needles\*.

To illustrate the operation of these apparatus, I shall state, that when the lower terminating wire, which was to the left, or east of the north pole, was connected with a piece of zinc, and the upper one with a piece of platinum, both being in common water, the deviation of the central needle was eight or ten degrees, the south pole turning to the east or left hand; which may be considered as indicating that the current of electricity was from the platinum to the zinc through the wire, and that the surface of the zinc in the fluid was positive with respect to the opposite surface of platinum; and in using the terms positive and negative, I beg to be understood as applying them to the metallic surfaces in contact with the fluid.

For determining weak electricities of charge, or as it is sometimes called, of tension, I used Volta's condenser connected with Bennet's electrometer, and sometimes with one constructed on the principle of Behrens, consisting of an in-

\* This arrangement differs from that of M. Nobile only by a duplication of effect.

sulated gold leaf, or what I found better, a silk filament, made conducting by impalpable charcoal powder, to receive the charge, placed between the poles of a dry pile consisting of 400 circles of silver and gold foil, of the third of an inch in diameter, or 50 of zinc and silver of the same size, with paper intervening; the attraction of the gold leaf or the filament, either to the positive or negative pole, indicates the nature of the charge: and, as in cases of electro-chemical action there are always two corresponding opposite states, I considered the part of the system which touched the conductor as possessing the same electrical state with that exhibited by the leaf. I have never however put much dependence upon indications given by this instrument, unless they were confirmed by other results; having found them very uncertain, and influenced by the state of the condenser and the atmosphere.

#### IV. *On the Electrical and Chemical effects exhibited by combinations containing single metals and one fluid.*

I know of no class of phænomena more calculated to give just views of the nature of electro-chemical action than those presented by single metals and fluids; and as their results are, with one or two exceptions, entirely new, I shall describe them with some degree of minuteness.—When two pieces of the same polished copper, connected with the platinum wires at the multiplier, were introduced at the same time into the same solution of hydro-sulphuret of potassa, there was no action; but if they were introduced in succession, there was a distinct and often, if the interval of time was considerable, a violent electrical effect—the piece of metal first plunged in being negative, and the other positive.

This result depends upon the circumstance of the production of a new combination, which is negative with respect to the metal; for after the formation of the sulphuret of copper, the plate of copper that has been first plunged into the solution exhibits the same negative state with respect to polished copper, whether introduced into saline solutions, or alkaline or acid menstua. The electrical effects therefore does not depend on so simple a condition as would at first appear, and it may be in fact referred to the combinations containing two metallic substances and one fluid.

The gray sulphuret of copper is negative, in solutions of hydro-sulphuret, to clean copper, and the superficial coating has apparently similar electrical powers to his substance.

Copper, in the state of protoxide, is negative, not only with respect to metallic copper, but likewise with respect to the sulphuret; a circumstance which explains many singular and apparently anomalous circumstances with respect to the action of hydro-sulphuret on copper. I have often found the order which I have mentioned, of metallic copper being positive with respect to copper that had been a few seconds in solution of hydro-sulphuret, reversed in a singular and capricious way; but on investigating the cause, I have often found that the copper was tarnished; and on heating any kind of polished copper strongly, so as to produce a thin coating of oxide any where on its surface, it became strongly negative to copper plunged in solution of hydro-sulphuret: the same effect was produced by the action of acids.

There are some singular circumstances connected with the violent and intense chemical action of copper on solutions of hydro-sulphurets, which are worthy of being described. When a piece of copper connected with the multiplier has been for a minute in strong solution of hydro-sulphuret of potassa, on introducing a piece of polished copper connected with the other wire, there is often a violent and momentary negative charge communicated to it, which sends the needle through a whole revolution: it then oscillates, and almost immediately returns, and takes the direction which indicates that the piece first plunged in is negative. This effect continues for some minutes, then becomes weaker; at last the two sides are in equilibrium, and the piece which was first plunged in now becomes positive with respect to the other. The first described of these effects seems to depend upon the discharge, by the clean copper, of the negative electricity accumulated by the contact of the plate first plunged in, before the relative states produced by the metallic contact and the regular currents occur; and the second, to the detaching or peeling off of the coat of sulphuret, which has the effect of exposing a clean surface, and which effect is probably occasioned by the oxidation of the positive side of the plate.

There are few electrical actions more intense than those produced by the operation of hydro-sulphurets on copper in these different circumstances; so much so, that I have constructed a Voltaic battery which decomposed water, by six combinations, consisting merely of thin slips of copper, of which one half had been exposed to the solution about a minute before the other half: of course, the oxidating surface was on the side of the clean or latest exposed metal.

With lead, and alloys of tin and lead and iron, there are the same phænomena, but much feebler electrical action, the metallic surface; which is first introduced being the negative surface; and the principles of this kind of action are precisely the same as those of copper and hydro-sulphurets.

Zinc, platinum, and metals which have no chemical action on solutions of hydro-sulphurets, produce no phænomena of this kind; silver and palladium, which acts powerfully with these menstrua, produce very decided effects; but the compounds they form in them being positive with respect to the pure metals, the phænomena are the reverse of those offered by the more oxidable metals; the surface plunged first into the solution is the positive surface, and it retains this relation in alkaline, acid, and saline solutions presenting peculiarities dependent upon the change of surface, which I shall refer to again hereafter.

The production of electrical currents by single metals and single fluids, though most distinct in the cases I have just named, yet occurs generally whenever new substances which can adhere to the metals are produced in chemical action. Thus in acid solutions of a certain strength pieces of the same zinc, tin, iron, and copper, exhibit similar phænomena; the surface first plunged into the acid being tarnished, or retaining a slight coat of oxide, is negative to the metal plunged in afterwards, and the relation is sustained in saline or alkaline solutions. The same effect is caused by producing a coat of oxide by heat on the surface, or even by applying it artificially. The oxidated surface is negative with respect to the other.

Zinc, which dissolves in a strong solution of potassa, giving off hydrogen copiously, exhibits exactly the same phænomena in this solution; the tarnished metal, or that first introduced, being negative with respect to the other. Tin likewise in solution of potassa, having been introduced long enough to have tarnished, is strongly negative with respect to polished tin.

Even the noble metals obey the same law. Silver, that has been tarnished by the action of nitric acid, is negative to polished silver in diluted acid; and gold and platinum, that have been acted on by aqua regia, are negative in that acid to the clean metals.

The intimate connexion displayed in all these cases between the chemical and electrical phænomena, becomes still more remarkable when the nature of the changes taking place in circles of this kind is considered.

Oxygen, which may be considered as negative with respect to all the metals, and sulphur, which is negative with respect to the oxidable metals, by their combinations with metals respectively positive to them, produce compounds negative with regard to those metals. And in the chemical changes, the results are such as must ultimately restore the equilibrium, hydrogen or sulphuretted hydrogen passing to the negative side, and oxygen to the positive side; so that the oxides are revived; and not only is the equilibrium restored, but the poles sometimes changed. Thus tin that has tarnished in acid, remains for some time negative in solution of alkali, but gradually as the oxide upon it is revived by the hydrogen determined to this surface, it loses its negative power; and the other surface, now tarnished by the action of the alkali, gains this power, whilst the opposite surface becomes positive.

*V. Of electrical combinations, consisting of two imperfect, and one perfect conductor; or two fluids and a metal, or charcoal.*

To understand clearly the nature of the action in this kind of electrical combination, it is necessary to consider the nature of imperfect conducting bodies, water, or saline solutions. These bodies may be regarded as having the same relations to electricities of very low intensity, that elastic fluids have to the electricities of glass, sealing-wax, or the common machine. They communicate the electrical polarities of the metals, but do not appear capable of receiving such polarities or at least of retaining them; and the electrical equilibrium, when broken in them, seems to be rapidly restored by a new arrangement or attraction of certain of their elements. For instance, if we introduce the positive and negative poles from a very powerful voltaic battery into the extremities of a basin filled with solution of muriate of lime, and place in the circuit different wires of platinum, every wire will possess a positive and negative pole, and there will be no division of the fluid into two parts, one positive, the other negative; and when the two wires are withdrawn, they alone having been used, the electrical appearances immediately cease; and metallic wires unconnected with the battery made to occupy their places, exhibit no electrical phenomena: and in all experiments of this kind, the well known phenomena of the development of chlorine and oxygen and acid matter at the positive, and hydrogen, alkaline matter, &c. at the negative pole, takes place.

Acid and alkaline matters, when perfectly dry and non-conducting, becomes on contact negative and positive; as I have shown is the case with oxalic acid and lime; but this

effect is similar to that of glass and silk, and the result is a common electricity of tension. And when acids and alkalies combine, their union being apparently the result of the same attractive powers acting on the particles which would produce their electrical relations as masses, they exhibit no phenomena of electro-motion; and such phænomena, when they occur in combinations in which acids and alkalies unite, always depend upon the contact of the metal with the acid and alkaline matter, change of temperature, evaporation, &c. and never on the combination of the acid and alkali.

As a different opinion has been lately started, on high authority\*, I shall give the proofs of the truth of this my early view, which appear to me of the strictest demonstrative nature.

A solution of nitre, which is a substance neutral to the contact of noble metals, was introduced into a glass cup containing a plate of platinum connected with the multiplier; pure concentrated nitric acid was placed in another cup, in which there was another plate of platinum joined to the other wire of the multiplier, and the connexion was made by a piece of asbestos wetted in a solution of nitre. At the amount of contact, the needle indicated a strong electrical action, negative on the plate plunged in the acid, and which occasioned a permanent deviation of about  $60^{\circ}$ .

This arrangement was removed from the multiplier, and another substituted for it, in which strong solution of potassa occupied the place of the nitric acid, being in contact with platinum in one cup, and solution of nitre in the other, with the same communications. The deviation was now much weaker, about 10 degrees, and the platinum in the solution of potassa was positive.

The nitric acid and the solution of potassa were now connected in the arrangement by a piece of clean asbestos, moistened in a concentrated solution of nitre; the deviation of the needle was to about  $65^{\circ}$ . In this instance there was no chemical action of the fluids on each other; for they had no tendency to mix rapidly with the solution of nitre, which being of less specific gravity than either of the other solutions, remained in the asbestos; and there was no effect beyond that of the metallic contact of the platinum with acid and alkali.

A piece of asbestos, of nearly the same size with the other, but dry, was now substituted for the moist asbestos, so that the acid and alkali combined by capillary attraction producing heat: at first, the deviation was rather less than in the former instance; but as soon as the combination was complete, the

\* That of M. Becquerel.

needle stood exactly at the same point, proving that no electricity was developed by the combination, any more than by the indirect communication of the acid and the alkali.

After trying the effects of the contact of fluid acid upon platinum by the arrangement with solution of nitre, and finding that oxalic acid was the acid among the powerful ones which produced the slightest deviation of the needle, or the smallest negative effect, I employed this acid and solution of potassa, exactly in the same manner as the nitric acid in the experiment just detailed; as the joint action of the acid and alkali on the platinum was only to produce a deviation of 7 or 8 degrees, it might be suspected that any electrical action produced by combination might be more easily manifested; but no such effect occurred; and whether the communication was made by combination through dry asbestos, or through asbestos wetted in a saline solution, the effect was precisely the same.

Again,—the two surfaces of platinum were placed in contact with strong solutions of nitre, and the communication made between them by solution of potassa and nitric acid; there was no electrical action, though the chemical combination was intense. But when the fluids were mixed, so that a little acid touched one plate of platinum and a little alkali the other, electro-motion immediately began; and in using muriatic acid and solution of ammonia, which, being lighter than the saline solutions, very soon come in contact with the platinum, the effect commenced almost immediately, and continued for some time to increase.

Again,—I placed pieces of paper coloured with litmus and turmeric, and moistened in solutions of nitre, upon two surfaces of platinum connected with the multiplier; they were covered with a stratum of porcelain clay wetted with the same solution, a stratum of clay moistened with muriatic acid was placed above on one plate, and a stratum moistened with solution of ammonia above on the other, so as to make a contact in which there should be action upon a large surface without direct communication with the metals. In several experiments of this kind there was no electro-motion; and whenever it was perceived, it was found that either the acid, or the alkali, or both, had penetrated through the clay, and touched the metals so as to change considerably the colour of the papers, which were placed as indications of the correctness of the experiment.

Having brought forward what appear to me decided proofs on this subject, I shall now proceed to investigate the opera-

tion of the metals and fluids in combinations containing two of the latter substances. At first I was surprised to find that platinum acted so powerfully with nitric acid, which undergoes no chemical change by contact with it, and suspecting that it might arise from the presence of minute portions of muriatic acid or muriatic salts, I took great pains to exclude these substances by washing the platinum in distilled water, not touching it with the hands, &c. but when the conditions were those of perfectly clean and pure platinum and perfectly pure nitric acid, the phenomena were the same. Similar reasonings may be applied to solutions of potassa, soda, &c., which do not chemically alter platinum by contact, and yet render it positively electrical with respect to platinum in water or saline solutions. It must however be called in mind that the oxygen in nitric acid, and the metals in the alkalies, have attractions of a very decided kind for platinum; and in taking the scale of electro-negative bodies, solutions of chlorine, or nitro-muriatic acid, produce a more powerful electrical effect on platinum than nitric-acid, nitric-acid than muriatic, and muriatic than sulphuric.

When platinum is brought in contact with an acid, the pole touching the acid is negative, the opposite pole is positive, as I have found by the condensing electrometer; and the reverse is the case when it touches an alkali; so that the circulation of the electricity is from the metal to the alkali, and from the acid to the metal.

Rhodium, iridium, and gold, act in combinations consisting of acid and alkali, on which they have no chemical effect, exactly like platinum; the surface of the metal in the solution of alkali being positive, that in the solution of the acid, negative. With silver and palladium the electricity is greater, particularly if nitric-acid is used; and with charcoal and oxidable metals, there is the same general result, the action being in general exalted in proportion as the chemical attractions are stronger, provided there are no interfering circumstances: and in combinations of this kind nitro-muriatic acid is more active than nitric, and the order is after, nitric, nitrous sulphuric, phosphoric, vegetable acids, sulphurous, prussic, sulphuretted hydrogen; and, with the alkalies, potassa, soda, baryta, ammonia, and so on.

It is always to be understood that strong or concentrated solutions of acids and alkalies are employed; for in cases where the quantity of acid or alkaline matter is very small and the chemical action of the metals strong, there is sometimes a different order. Thus zinc and tin tarnish immedi-



ately even in a weak solution of pottassa, and, so tarnished, they are negative to the same metals in weak solutions of muriatic or sulphuric acid; but in experiments of this kind it is easy to determine the true circumstances by changing the poles; the negative side, when the energies of the alkali and acid are weak, will be determined by the tarnish or coat of oxide formed.

Solutions of sulphurets act in these combinations like alkali, with circumstances depending upon the formation of new compounds, according to the law explained in the last section. In combinations, of which the elements are hydro-sulphuret and acid, the metal in the hydro-sulphuretted solution is positive, and that in the acid negative; but with alkalies and hydro-sulphurets, and zinc, and tin, the metal in the solution of alkali is positive, and that in the solution of hydro-sulphuret, negative: with silver and palladium the opposite order occurs, and with copper there is nearly a balance of powers, or changes of power, dependent upon the circumstances detailed in the last section.

When, in electrical combinations containing one metal, water or a neutro-saline solution is in one of the cups, and alkali or acid in another, the result is usually such as might be anticipated,—the side of the metal in the alkali is positive, that in the acid negative, and that in the neutro-saline solution in the opposite state. There are however certain neutro-saline solutions, which when they contain oxygen or common air, act upon the more oxidable metals, and such have a power or energy of their own; thus zinc, and tin, and copper in solution of common salt, are positive to the same metals in distilled water; and the surfaces of the same metals in weak muriatic acid are positive with respect to the surfaces in water or saline solutions. In combinations, in which weak and strong solutions of acid or of alkalies are the two fluids, both being of the same kind, the electrical action is usually feeble; but the surface in the strongest alkali is most positive, and in the acids the result usually depends upon the nature of the solution; if oxide is formed and deposited, the strongest acid is negative with respect to the diluted one.

The chemical changes produced in combinations of this kind, are best observed in cases where the metal undergo no change; for instance, with platinum, diluted sulphuric acid, and solution of potassa. In this combination, hydrogen soon appears on the platinum in the acid, and a very small quantity of gas, which is probably oxygen, on the platinum in contact with the alkali; and that the acid tends to circulate towards

the negative surface, and the alkali towards the positive, is shown by the circumstance of the rapid neutralization of the two menstrua, though separated by asbestos moistened in distilled water.

VI. *Of combinations consisting of two conductors of the more perfect class, and one fluid.*

The order in which metallic bodies exhibit electricities on contact, as is well known, is intimately connected with their relative oxidability, the most oxidable metal being positive with respect to all those below it. This law extends likewise to the newly-discovered bases of the alkalies and earths. Potassium and sodium, as I have found by bringing them in contact with zinc in a concentrated solution of alkali, are apparently as much positive with respect to this body, as zinc is with respect to platinum and gold.

There is not however any inherent and specific property in each metal which gives it the electrical character; it depends upon its peculiar state—on that form of aggregation which fits it for chemical change. Thus, zinc in amalgamation with mercury is positive with respect to pure zinc, and the amalgam of tin is in the same state with regard to tin; and the metals of the fixed alkalies in amalgam give the highest positive energy to a mass of mercury some thousands of times their weight.

In general, the electricities developed by metallic contact are of a stronger kind than those resulting from the contact of metals with fluids, so that they are not capable of being changed by them. For instance: zinc in acid is positive with respect to all other metals below it in degree of oxidability, though they are placed in alkalies or solutions of sulphurets: there are however exceptions; for instance, with regard to tin, which, when in a strong solution of potassa, is positive to zinc, in an acid solution; and with respect to iron, which, though positive with regard to copper in all acid or neutro-saline fluids, is negative to it in solution of sulphurets or of alkalies. The electro-motion in these instances produced by the contact of the fluids prevailing over that produced by the contact of the metals.

And knowing the energies of the acid and alkaline fluids, it is easy to apply them so that to diminish or enhance the electrical effects developed by metallic contact.

If, for instance, in a combination containing zinc and platinum, we use two fluids, and place the acid in contact with

the zinc, and the alkali with the platinum, the effect will be exceedingly feeble compared with that produced, if the order be reversed, and the zinc be in contact with the alkali, and the platinum with the acid.

The chemical changes taking place in combinations of this kind are always such as tend to restore the equilibrium; the hydrogen and the alkaline body always passing to the negative, and oxygen and the acid to the positive metal.

There is no instance of continued electro-motion except in cases where chemical changes can take place, for even De Luc's or Zamboni's columns do not act when quite dry, and the silver in combinations of this kind, when the negative metal is gold, is uniformly found tarnished: for the exhibition of electricities of tension, however, a very slight chemical action is sufficient, as the quantity of electricity required to give repulsion to light bodies is exceedingly small; but to form electro-magnetic combinations, the chemical agents must be of an energetic kind.

As most of the fluids which act powerfully in voltaic combinations contain water, or oxygen and hydrogen, it has been suspected that these principles were essential to the effect: this however, does not seem to be the case, for I found zinc and platinum formed powerful electro-motive circles in fused litharge and fused oxy-chlorate of potassa, which are not known to contain water; and I have little doubt that similar effects would be produced by other fused salts containing only acid and alkaline matter.

It may elucidate this part of the subject, which must at best be obscure, to take a view of the changes occurring in one of the simplest voltaic combinations—that consisting of zinc, platinum, and solution of sulphate of soda. It is a fact that zinc and platinum become electrical by contact, the zinc positive, the platinum negative; and the two kinds of electricity are apparently most intense at the surfaces where they are in contact with the fluid, which is too imperfect a conductor to allow them to neutralize or destroy each other; they consequently exert their attractive and repellent powers upon the elements of the menstruum; acid and oxygen circulate to the surface of zinc, which in consequence is dissolved, and alkali and hydrogen to the surface of platinum, of which the hydrogen is disengaged, and the equilibrium broken by the contact of the metals is restored by the chemical changes; so that a constant circulation, or a current of electricity, takes place, the power of the combination becoming feebler in proportion

c.

as the solution is decomposed, and acid accumulated round its positive, and alkali round its negative surface.

In cases where acids or acid solutions alone are used, the destruction of one or both surfaces, with the transfer of hydrogen or oxygen, seems to produce the same effect; and the inactivity of single circles or voltaic piles, in which pure water is used, or saline solutions freed from air, seems to show that the destruction of the surface of the oxidable metal is one of the conditions of continued electrical action; and the cessation of the power of De Luc's or Zamboni's piles, is always connected with the tarnish of the imperfect metal employed in them.

Having published, many years ago, tables of the electro-chemical relations of metals, which have been copied into many elementary books, I think it proper to give them here in a corrected form, with some additions, and the differences dependent upon the nature of the menstruum. The metal mentioned first is positive to all those below it in the scale.

*With common Acids.*

Potassium and its amalgams; barium and its amalgams; amalgam of zinc; zinc; amalgam of ammonium(?); cadmium, tin, iron, bismuth, antimony(?), lead, copper, silver, palladium, tellurium, gold, charcoal, platinum, iridium, rhodium.

*With Alkaline solutions.*

The alkaline metals and their amalgams: zinc, tin, lead, copper, iron, silver, palladium, gold, platinum, &c.

*With Solutions of hydro-sulphurets.*

Zinc, tin, copper, iron, bismuth, silver, platinum, palladium, gold, charcoal.

VII. *On the accumulation of Electricity, and the chemical changes it occasions in voltaic arrangements.*

In the view of electro-motion adopted by the illustrious inventor of the pile, the metals were considered as the *only* agents which, in proportion to their surface and their number, occasioned the constant circulation of a certain quantity of electricity through the fluids, or the connecting wires in the pile; and the chemical changes occurring in these fluids, were considered as mere results, and not necessarily connected with the circulation. The inactivity of combinations

where no chemical changes occur, is sufficiently hostile to this view; but an examination of some of the circumstances of the construction of compound electrical combinations, will bring this hypothesis, and that which I have ventured to adopt, more distinctly into comparison.

Let a piece of zinc and a piece of platinum, both in glasses filled with a solution of nitrate of potassa, be connected through the multiplier, and let the glasses be joined by asbestos, moistened with the same fluid; the needle will mark electrical action: let the two glasses now be joined by an arc composed of zinc and platinum, in such a manner that the order is Voltaic, *i. e.* that the zinc is opposite to the platinum, in the original combination—the effect will be increased. Now let an arc of pure zinc be introduced; the effect will be less than with the double arc, but superior to that with the asbestos, and the pole of the zinc opposite the platinum will oxidate, and that opposite the zinc will give off hydrogen. Let arcs of other metals be substituted for the zinc; for instance, of tin, of iron, of copper, of silver, of tellurium: the electrical effects will diminish with the oxidability of the metal; and with tellurium, which does not oxidate at the positive pole of a voltaic battery, they will be destroyed; and the case is the same with rhodium, palladium, and platinum. That the effect does not depend upon any circumstance connected with conducting power is evident; for charcoal, which is a very imperfect conductor, acts like an oxidable metal; and a very fine wire of platinum, terminated by a small piece of oxidable metal, acts more efficiently when the oxidable metal is opposite the negative pole, than if the whole chain had been composed of oxidable metal; but entirely destroys the effect when the oxidable metal is opposite the positive pole.

If the contact of the metals only was necessary for continued electro-motion, these results, in which a simple homogeneous chain is interposed between the fluids, would be impossible; but they are a necessary consequence of the electrochemical theory, in which the destruction of the positive surface, by the chemical negative agent is regarded as a necessary condition; and platinum and tellurium acted like zinc, when their surfaces opposite to the platinum were plunged into diluted nitro-muriatic acid.

If two, three, or four glasses are used, and two, three, or four arcs of platinum and zinc, the extreme metals of which are connected through the multiplier, a piece of platinum used instead of one of the arcs will not now entirely destroy the electro-motive effect: it will be diminished as if one arc

had been removed. The two will act as a single combination; the three as two arcs, and the four as three; and of course in a voltaic combination of 100 arcs, a single piece of platinum substituted for any one of the arcs, will diminish the power of the apparatus only 1-100th part.

In attempting to protect copper by zinc, in a separate vessel, from the action of sea-water, I found that when the two vessels were connected by moist tow or vegetable substances, or by a wire (even through fine) of any oxidable metal, the protection was complete: but when even a thick wire of platinum was employed, the copper, though in immediate contact with the zinc, became corroded. After the experiment had continued several days, the surface of the platinum opposite to the copper was found tarnished, as if it had been slightly acted upon by the chlorine combined in the sea-water; but this effect had been too feeble to be connected with any sensible degree of electrical polarity in the platinum.

This result, with those mentioned in the preceding pages, seems to show that there can be no accumulation of electricity in voltaic combinations, unless the same or similar conditions of chemical change exist in the elements or single circles composing them; and that under other conditions, the power generated in single circles is either destroyed or diminished according to the opposing nature, or want of conducting power of the chain of intervening bodies. For instance, in the arrangement (mentioned in p. 191) of one piece of zinc and one of platinum, the power is doubled by another series of the same kind, destroyed by an arc of platinum, and diminished by an arc of zinc; by a second solution and a second arc of zinc, it is diminished still more; by a third it is nearly, and by a fourth absolutely, *destroyed*.

As the chemical changes always tend to restore the electrical equilibrium destroyed by the contact of the metals with each other in the fluids, it is evident that in case in which arcs primarily inactive are connected with those primarily active, the chemical changes produced by the electrical attractions must tend to produce in the primarily inactive parts of the combination, an arrangement which must give it a power in direct opposition to that of the primarily active circles; so that when separated, their actions, if any, must be directly the reverse of the other. This result, which I anticipated, I have actually found to be correct; six arcs of platinum in vessels filled with solution of nitre, were connected with a voltaic battery of 50 pairs of plates; of course each arc gave off oxygen, and collected acid round the pole in the place of the zinc, and afforded hydrogen and collected alkali round the

pole in the place of the noble metal: on separating the six arcs from the battery, they were found to possess independent action, the poles which were negative being positive, and those positive being negative: in short, the combination acted as if an original one, consisting of acid, alkali, and platinum.

With arcs of zinc, the results were of the same kind, but the electrical effects were much more distinct: as the tarnished zinc in this case, added its own negative powers to that produced by the contact with the acid.

In trying similar experiments with six arcs of tin, silver, copper, and other metals, and using different saline solutions, it was found that the reversed electrical effects were most powerful with the most oxidable metals, and the most concentrated and most decomposable solutions; and the weakest arrangement of this kind, was with arcs of platinum and pure water; yet even in this instance the water had become slightly alkaline at one pole, and acid at the other.

These experiments, showing the nature of the chemical changes in combinations made active by their connexion with voltaic batteries, and the influence of the newly-developed chemical agents, fully explain the phenomena of the secondary piles of M. Ritter: and combined with the fact, that the metals are not *perfect* conductors for electricities of very low intensity, they offer a simple and adequate solution of the circumstance observed by M. De La Rive on the interposition of different metallic plates in the fluids connecting together voltaic combinations.\*

From the nature of the chemical changes taking place in each single circle of a common voltaic battery, it is evident, that if any small part of a battery for some time in action, is separated from the whole, and made to act as a distinct combination, its powers must be feebler than if it had been originally an independent series; for the electrical action occasioned by the chemical agents developed in it, are such as to counteract the effects produced by the contact of the metals. Whereas, if a small voltaic series is connected with a much larger one, in reverse order, its oxidable in the place of the noble metals, though the whole power of the combination is much weakened by it when in union; yet, when separated, it must act with much greater power, as the chemical changes produced are exactly of the kind which must enhance the primary power of the metals. This deduction (a necessary consequence of the electro-chemical theory) I have proved by

\* *Annales de Chimie et de Physique*, tom. xxviii. p. 190.

direct experiment. A series of 6 arcs, composed of zinc and copper and solution of nitre, was connected in the proper order with the voltaic arrangement of 50 pairs, and suffered to remain in connexion for 10 minutes; they were then separated, and made to act as a single battery: their powers were extremely feeble, not certainly one-third as great as those of a combination of the same kind which had been in action (but unconnected) for the same time. Six arcs of copper and zinc were now connected with the same battery of 50, in a reverse or unconformable manner, so that the six plates of zinc gave off hydrogen and attracted alkali, and the plates of copper oxidated and attracted acid. Being separated after a few minutes, and made to act alone, they exhibited powers which appeared three or four times greater than if they had never been in connexion; the zinc assumed a much higher positive, and the copper a higher negative state, than if they had not before been in the antagonist or unconformable conditions.

All these facts bear upon the same point, and confirm the view which I took of the nature of voltaic combinations in the Bakerian Lecture for 1806; in all of which, whether the destruction of the electrical equilibrium is produced by the contact of metals or fluids, it is always restored by chemical changes, and in which the circulation, if it may be so called, depends upon a union of these causes, the direction of the currents being always opposite in the metallic and fluid parts of the combination, so as to produce what may be regarded as an electrical circle.

#### VIII. *General observations and practical applications.*

To explain the manner in which different chemical agents in combination, and in a perfectly neutral state, instantly start into an active existence, when exposed to the two electrical poles, it is necessary to assume principles, and take views of corpuscular action of a perfectly novel kind; and as the chief agents are invisible, and probably imponderable, no direct demonstrative evidence can be brought forward on the subject; and different hypothesis may in consequence be applied to it. In assuming the idea of two ethereal, subtile, elastic fluids, attractive of the particles of each other, and repulsive as to their own particles, capable of combining in different proportions with bodies, and according to their proportions giving them their specific qualities and rendering them equivalent masses, it would be natural to refer the action of the poles to the repulsions of the substances combined with excess of one fluid, and the attractions of these united to the



excess of the other fluid ; and a history of the phænomena, not unsatisfactory to the reason, might in this way be made out ; but as it is possible likewise to take an entirely different view of the subject, on the idea of the dependence on the results upon the primary attractive powers of the parts of the combination on a single subtile fluid, I shall not enter into any discussion upon this obscure part of theory, but I shall endeavour to clear the way for elucidations of it by stating some experimental results.

Some solution of nitre of patassa was introduced into a glass basin of six inches in diameter, and a large slip of paper, tinged with litmus and turmeric, were placed below the fluid, and connected with two pieces of foil of platinum ; so that the indications of the formation of acid and alkali, in any part of the basin, by electricity, would be instance and distinct. The two pieces of foil were now connected with the poles of a voltaic battery : it was found that the alkali was developed only at the point or immediate surface of the negative platinum, and the acid in the same manner at the surface of the positive platinum ; and they then gradually diffused themselves through the fluid in a circle round the conductors, and there was no appearance of any repulsions or attractions of the menstrua in the line of the circuit.

In various repetitions of this experiment the same result was obtained ; the alkaline and acid matters were influenced in their direction only by currents produced by the disengaged oxygen or hydrogen, or the inclination of the vessel ; in short, by mechanical causes only ; and the same effects were produced on the test papers, as if a spherical piece of acid and an amalgam of potassium had been introduced in the places of the two poles.

Mr. Herschel has shown, by some elaborate and ingenious experiments in the last Bakerian Lecture, that an amalgam of potassium, containing so minute a portion as some hundred thousand parts of its weight is strongly attracted so as to occasion violent mechanical motion, by the negative pole in a voltaic arrangement : and if it be supposed that the fluid is divided into two zones, directly opposite in their powers to the poles of the battery, the virtual change may be regarded as taking place in the two extremities of these zones nearest the neutral point ; so that by a series of decompositions and recompositions, the alkaline matters and hydrogen separate at one side, and oxygen, pure or in union, at the other.

In this way, the two electricities may be regarded as the transporters of the ponderable matters, which assume their own peculiar characters at the moment they arrive at the point

of rest. I shall detail an experiment which I made under a different form some years ago, and which may assist the imagination in the conception of this singular and mysterious mode of action. A flat glass basin, 10 inches in diameter, was filled with water containing 1-2000th part of its weight of sulphate of potassa, in the bottom of which are 30 or 40 separate globules of mercury, containing from 10 to 100 grains each, were placed without any regard to order; two wires of platinum from a battery of 1000 double plates, weakly charged, were made to connect the extremities of the water, (passing to the bottom of the basin). As soon as the electrical communication was made, the globules of mercury in or near the current became instantly agitated; their negative poles became elongated, and approached either the positive pole of the battery, or the positive pole of the contiguous globules of mercury, and streams of oxide flowed with great rapidity from the positive toward the negative pole. No hydrogen appeared at the negative poles of the globules of mercury; but after the action had continued a few minutes, and was then suspended, there was an appearance of some minute globules, owing, as was proved by tests, to the formation and oxidation of potassium which had combined with the mercury, and which, as is evident from Mr. Herschel's researches, had given to that part of the globule in which it had combined its high electro-positive qualities. When the connexion was again made, the same series of constant and violent motions took place; the elongated and negative extremities of every globule moving towards the positive surfaces, and undergoing continual oscillations; but on pouring a small quantity of muriatic acid into the water, so as to make it slightly acid, these phenomena ceased; the masses of mercury resumed their spherical form, hydrogen was given off from the negative surfaces, and all motion and agitation were at an end. The energy of the acid in this case being negative, may be considered as neutralizing the power of the potassium by its immediate contact, and as destroying all the phenomena of attraction by the positive pole.

In the numerous experiments that I made in 1806, on the transfer of acids to the positive pole and of alkalies to the negative pole, there were similar instances in which masses of acid or alkaline matters, by exerting their own peculiar energies, prevented the accumulation of the antagonist elements at their points of rest, so as to destroy, or materially weaken, their power of motion or transport. For instance, in attempting to transfer baryta from the positive to the negative pole, the negative pole being plunged in sulphuric acid, or sulphuric

acid to the positive pole, the negative being plunged in a solution of baryta, the re-agents were neutralized, and formed insoluble precipitates at the point of union of the menstrua; and no baryta reached the negative, and no sulphuric acid the positive pole.

With muriatic acid and salts of silver the case was the same. And when acids and alkalies, forming soluble compounds, were used in similar experiments, a great length of time was required, proportional in some measure to their masses, before a particle of acid reached the positive, or of alkali the negative pole; and the result was not destroyed till after the intermediate combination had taken place to a considerable extent; proving the phenomena of continued decompositions and recompositions, and showing that the electrical and chemical phenomena are of the same order, and produced by the same cause.

In the Bakerian Lecture for 1806, I proposed the electrical powers, or the forces required to dis-unite the elements of bodies, as a test or measure of the intensity of chemical union. By the use of the multiplier, it would be now easy to apply this test; and *accurate* researches on the connexion of what may be called the electro-dynamic relations of bodies to their combining masses or proportional numbers, will be the first step towards fixing chemistry on the permanent foundation of the mathematical sciences.

I could enter into some other general views of the pure scientific relations of this subject, and its connexion with thermo-electricity and the phænomena of cohesion; but having already taken up so much of the time of the Society, I shall defer what I have to say on these subjects to another occasion, and I shall conclude with a few practical observations.

A great variety of experiments made in different parts of the world has proved the full efficacy of the electro-chemical means of preserving metals, particularly the copper sheathing of ships; but I hope I had once indulged, that the peculiar electrical state would prevent the adhesion of weeds or insects has not been realized; protected ships have often indeed returned after long voyages perfectly bright,\* and cleaner than unprotected ships, yet this is not always the case; and though the *whole* of the copper may be preserved from chemical solution in steam-vessels by these means, yet they must be adopted in common ships only, so as to preserve a portion—so ap-

\* The Carnegrea Castle.

plied, as to suffer a certain solution of the copper;\* and an absolute remedy for adhesions, is to be sought for by other more refined means of protection, and which appear to be indicated by these researches.

The nails used in ships are an alloy of copper and tin, which I find is slightly negative with respect to copper, and it is on these nails that the first adhesions uniformly take place: a slightly positive and slightly decomposable alloy would probably prevent this effect, and I have made some experiments favourable to the idea.

In general, all changes in metals which would indicate the power of chemical attraction, are easily determined by electrical means. Thus I found copper hardened by hammering negative to rolled copper; copper (to use the technical language of manufacturers) both *overpoled* and *underpoled*, containing in one case probably a little charcoal, and in the other a little oxide, negative to pure copper. A specimen of brittle copper put into my hands by Mr. Vivian, but in which no impurity could be deduced, was negative with respect to soft copper.

In general, very minute quantities of the oxidable metals render the alloy positive, unless it becomes harder, in which case it is generally negative. As I have mentioned before, amalgams of the oxidable metals are usually positive, not only to mercury, but even to the pure metals.

There are probably few chemical operations which electrical changes do not influence, and either increase or modify. In the rusting of iron for instance, the oxide formed by the contact of moisture becomes the negative surface, and exalts the oxidability of the mass of metallic iron, and the rust consequently extends in a circle.

The precipitations of metals have been already traced to causes of this kind, and many metallic solutions must belong to the same order of phenomena.

I have pointed out in former papers some of the cases of electro-chemical protection, which I have no doubt, when the principles are well understood, will be generally adopted; and others are constantly occurring. I shall mention one—the preservation of the iron boilers of steam-engines by introducing a piece of zinc or tin. This in the case of steam-boats, particularly when salt water is used, may be of the greatest

\* A common cause of adhesions of weeds or shell fish, is the oxide of Iron formed and deposited by the protectors. In the only experiment in which Zinc has been employed for this purpose in actual service, the ship returned after two voyages to the West Indies, and one to Quebec, perfectly clean.

The experiment was made by Mr. Lawrence, of Lombard-street, who in his letter to me states that the rudder, which was not protected, had corroded in the usual manner.

advantage, and prevent the danger of explosion, which generally arises from the wear of one part of the boiler.

Another application of importance which may be made, is the prevention of the wear of the paddles or wheels, which are rapidly dissolved by salt water.

But I will conclude. Whenever a principle or discovery involves or unfolds a law of nature, its applications are almost inexhaustible; and however abstracted it may appear, it is sooner or later employed for common purposes of the arts and the common uses of life.

*On the influence of the lamellated state, in the phenomena of polarization and of double refraction produced by different crystalized bodies. By M. Biot. \**

Being desirous in the following remarks, to separate and class different orders of facts, which appears to me not to have been done hitherto, sufficiently to distinguish the one from the other, I shall commence by defining them distinctly in precise terms, in order that they may not have to be further pointed out hereafter, in the applications, by the characters which are proper to them. In Physical mineralogy bodies are considered as continually crystallized, by the aggregation, real or ideal of an infinity of solid particles of insensible dimensions, having for each chemical substance, whether simple or compound, a special configuration; and put together at equal distances, in such a manner that their homologous faces are all parallel amongst them. Are these elementary solids really of the supposed configuration? or would they not express the distribution resulting from the attractive forces, exercised by the true constituent particles. We are ignorant of the fact; but with this reserve dubitative—their conception may always be substituted for realities. Haüy has named them *the primitive forms* of crystals of each substance, or of each crystallizable combination. Perhaps it will be more exact to name their generated forms. For the special character which is attached to them, is that their sole opposition, regularly extended in diverse directions, re-produces to our senses, all the varieties of configuration of the crystals of the same substance; because the minuteness of the elementary solids renders the indentations on the surfaces, which limit the aggregate solids inappreciable, and thus assimilate them

\* Comptes Rendus.

to faces, in apparent planes, of crystalline *polyedrons*. In these terms of approximation, if we only wish to obtain a geometric and external representation of these polyedrons, we might construct them of any like form whatever. For their exterior configuration being assigned, we shall always find a mode of opposition of elementary solids which are similar to them. But, in a similar hypothesis of uniformity, the laws of this construction would be found almost always excessively complex; and besides, the purely geometric principles which would place these elementary solids in their relative situation, would not present generally any index of a physical cause, which might with plausibility, bring them to each other, or maintain these in them in an aggregated form, as we see takes place in the act of crystallization. It is thus infinitely more convenient, under these considerations, to choose for each substance a generic form—such as the resultant forms observed in deducing them by a mode of opposition which their physical constitution discloses, or renders at least very likely; and which applied to elementary solids, may, by its agreement with their mode of mutual attraction, justify almost to demonstration, the spontaneity, the symmetry, as well as the permanence of their arrangement. Now, these favourable conditions of choice are indicated, in a great number of cases by the relation of position, which the polydric faces of the crystals of each substance present, as also by the constant direction of cleavage; following which, the different portions of their mass may be more or less easily separated by plane sections. In rendering available all these indications, with a sagacity to which sufficient justice is, perhaps, at the present day not rendered, Haüy has found for all the substances, observed in the crystallized state, generic forms limited to five of the most simple polyedrons in geometry; and by decomposing these five forms, for the facility of calculation, into three still more simple polyedrons, he has attained the art of re-producing with these, all the varieties of the most complex crystals: by the laws of opposition, the expression of which is always restricted, solely to primary terms of the series of entire numbers or their relations, which present a singular anomaly—and perhaps a very profound one—with the numerical simplicity of ponderal proportions after which they operate by the best defined chemical combinations.

I am not ignorant of the fact, that since the death of Haüy, the principles of his method of crystallography have been almost entirely abandoned. In place of seeking, as he did, to devise as he did, the generic forms by a material decompo-

sition, more or less realizable of each crystal, in order to derive therefrom the complex forms, by the special law of opposition called *decroissements*, we have taken a course absolutely the reverse. We have defined crystals by the sole conditions of direction, and of relative situation which their external faces present, ideally prolonged to their common intersections on certain planes called axes. This construction applied to all known crystals, had made it to be seen that the faces thus prolonged, being classed after their geometrical analogies, always endeavour to compose themselves in a small number of standard polyedrons, which are defined by the mutual inclination of their axes, and by the relations of lengths comprised on these axes between the intersection of the prolonged faces which constitute them. Then a complex crystal being given, we have only to observe the relative positions, and the mutual inclinations of the surfaces which terminate it. With these elements, and the employment of the conditions of symmetry to complete it ideally, if that is necessary, the analytical calculation makes known the faces, which being prolonged belong by relation to the same type, as well as the different types which the crystal resembles; which attaching themselves to each other by the condition, that the length of their analogous axes, ought always to be among them in certain relations, and generally simple ones. The whole of these results evidently give the definitive description of this proposed crystal. This method, which had its birth in Germany, is at the present day almost generally adopted in France by young crystallographers. Its application is direct; and by considering crystals only by their exterior forms, it employs for data those elements, alone, which in most cases, can there be effectively observed. But by that alone, it furnishes no light on the intestinal constitution of each crystal, no more than on the mechanical mode of its formation. Now here, as in the study of all other natural products, this mechanism is precisely the most essential point to discover, because it depends on the molecular actions, which on all sides present themselves, at the present day, to our researches, as the grand mystery which it behoves us to penetrate. It is for this reason, that after having found the descriptive definition of a crystal by the German method, if we judge it actually the most convenient for this use, it will always be necessary to complete these exterior indications by a study of the interior constitution, founded on all the processes of exploration which physical and mechanical philosophy can furnish. There is no doubt but that an intelligent and persevering mind undertaking this task, would be conducted to important discoveries.

But, by omitting it, as is at the present day only too much the case, it is much to be feared that we only take the superficies for the foundation and main point of things. Such is at least, the ordinary consequence of scientific methods which have become predominant during a time. Others make amends for them in their turn; until a happy alliance takes place which makes them concur to a common end. But this re-union is never brought about by the first inventors, who are always exclusives, nor by their first disciples, amongst whom confidence forestalls examination. Many generations must succeed before we return to this simple rule of good sense, that in studying the works of nature, our feeble intelligence will not be too much assisted by all the helps we have it in our power to collect. For the end that I have here in view, I have no need to examine the connexion more or less certain, which may exist between the artificial representation of crystals, by the forms generic and their veritable physical constitution. The uniformity of this constitution, in the whole mass of each crystal continually constructed, is the sole character that I wish to attribute to them. Now it is rigorously established by the following fact, that it is the most general expression. If, in a crystal of a continuous construction, we insulate a solid of any sensible dimension, and any configuration whatever, all the similar solids, and parallel to it, which we may be able to extract from the mass of the crystal, will be physically and chemically identical with it. If one of them should act on polarized light following certain laws, if it exercise simple refraction or double refraction, be it attractive or repulsive at one or two of its axes, all the solids similar to it, and of a parallel configuration, will possess, following the homologous directions, similar properties to which I shall attach henceforward the epithet of *molecules*, to express that they belong generally to the system of constituent particles of the crystal, in their actual state of aggregation regularly continued. This uniformity of construction is not always maintained throughout the interior of crystalline masses; it is rarely the same; and for this reason we may conclude with Wollaston, that continuous crystals are only of infinitely small extent. Sometimes the change is manifested in a hasty manner, between certain parts of the mass having sensible dimensions, and which individually considered, present a similar mode of aggregation and distinct construction. Then the mass is composed of crystals of the same nature, which are mutually joined or penetrated; and we may apply to each of them the same definition—*molecules*. This is the case with crystals watered or aggre-



gated. We meet also with masses of crystals where this association of divers constructions vary with so much discontinuity, that it is difficult to discern the individual state of their sensible parts; then, the total system is called confusedly crystal. But the individual properties or molecules, are again discovered in the smallest fragments, reduced, if it be necessary, to microscopic dimensions; and at least their isolated existence there is always supposable, for we render them sensible, or we give them a new birth, when the whole mass is dissolved, and submitted to a new crystallization brought about by the action of heat.

Besides these molecular properties, thus defined by their nature and the continual operation of their laws, the crystalline masses sometimes possess other properties, in their aggregation, and which are there accidental; such for example as will occasion a general state and force of compression or expansion, which will persist in maintaining itself. A similar state produced on polarized light from effects which may be associated with molecular refraction, simple or double as in reality they may exist without this last. But we recognize them by their proper laws; and we discern also the part that we ought to attribute to them in the resulting effects.

In fine, the elementary solids of a crystalline mass, in always remaining parallel one to another, may occasionally aggregate themselves in plane plates, continual, and superposed in a manner more or less intimate; sometimes distinct to our senses, at other times solely by polarized light, which modifies itself while traversing their intersicés. This lamellated disposition manifests itself with evidence in the crystals of alum which contain ammonia. I have recently exposed the principle physical laws of effects which it produces on polarized light, laws of which the specification is then facilitated, because they are found associated with simple refraction. But the same lamellated disposition, and the same effects, may also be associated with the double molecular refraction, of which they modify the proper characters, as I shall hereafter show by examples. It becomes necessary then to distinguish them from this refraction by the dissimilarity of their laws, in order to appreciate with justice the particularities of phenomena which belong to the molecular constitution of the crystal, and those which result from the lamellated state considered abstractedly from this constitution.

The phenomena of polarization, and of simple or double refraction, observed in all crystallized bodies, where this lamellated state does not exercise any sensible action, have given to mineralogy a very valuable relation among the primitive forms of crystals and their action on light. According to this law, crystallized bodies exercise simple refraction, or the double molecular refraction, be it of one axis, or be it of two axes, according as their primitive form is symmetric about a point, a rule, or a plane. Solely in this latter case, the symmetry of the position of the plan may exist for the solid itself, adopted as a primitive form or for one of its crystallographic derivatives; and the two axes are directed in their plane in such a manner as to make equal angles with the faces of the solid simple or complex. No person has given more elements than Dr. Brewster, to establish this law; and no person likewise has given so much which seems to invalidate it.

In a memoir, inserted in the Philosophical Transactions of Edinburgh for the year 1816. Dr. Brewster announces that the crystallized muriate of soda, the diamond, and fluor spar meet together in nature under three different states: sometimes only exercising simple refraction, sometimes the double refraction, either attractive, or repulsive; these three states being able to co-exist, and to succeed each other alternately, in different parts of the same mass. This consequence appeared to him to result from modifications which he had seen to prove the polarized light, in traversing different crystals of the substances which I have just named. He infers therefore generally, that the substances of which the primitive form is a regular octohedron or a cube, compose a special class of bodies, which an occasional modification of elementary solids can successively put in physical conditions so very dissimilar. After having described, the very feeble effects thus brought about by fluor-spar, where the planes of the axes of double refractions, seemed to be parallels to the faces of the cubes, as in mineral salt, he adds, without any other detail, that he has seen similar phenomena produced by large pieces of transparent alum. The brevity of this indication of the fact, joined to the assimilation which it expresses renders it very probable, that chance alone presented it to the observation of Dr Brewster, crystals of alum different to those of our manufactures, where ammonia enters as an element. For these produce effects of such intensity, especially when their volume is rather considerable that he could not but of necessity have been struck

with it, and investigated its laws, which are very different from those which he has announced.\*

Three years later, in a memoir inserted in the 1st. Vol. of the Philosophical Journal of Edinburgh, p. 1, Dr. Brewster announces that the mineral known under the name of apophyllite can affect three different crystalline forms: the first exercising double refraction at one axes, the second at two axes, and the third presenting a mode of aggregation regularly complex, where these two sorts of double refraction shewed themselves associated in different parts of the whole mass. He renewed this subject in a more extended memoir, inserted in the Transactions of the Royal Society of Edinburgh for 1823 page 317. The generosity of his scientific friends having placed at his disposal several hundreds of crystals of apophyllite, as many incomplete as complete, possessing all the possible varieties of form, and provided from all the localities where this mineral is found, he applied to them new processes of observation, even microscopic ones. Not only did he again find the primary phenomena which he had discovered, but the complete and limpid crystals which he was enabled to study under every position, presented him with others still more extraordinary, which he has described and expressed with all their particularities. His conclusions were then partly the same as in his first work; that is to say, that he had there varieties of apophyllite of one axes, of two axes, and others in the form of tesselite. But amongst these he distinguishes one so complex, that it seemed to him to be constituted by forms unknown in crystallography, and of which the greatest liberty of imagination did not suffice to account for. Here, as in the memoirs of 1816 and 1819, the number as well as the situation of the axes, was not established on the duplications of images effectively observed, but on the modifications produced by polarized light when it traverses the crystals, following different directions without undergoing a sensible rectilinear doubling.

The announcement of these results was sufficient to make a great sensation amongst minerologists, who had found the

\* To avoid all misunderstanding, here is the textual phrase of Dr. Brewster:

"*Similar phenomena were exhibited in large pieces of transparent alum.*" (page 4.) He describes the sense of the effects in the three substances by saying (page 5.) "that the neutral axes coincide with the faces of the cubes, and the depolarizing axes with their diagonals." Now that which Dr. Brewster calls the neutral axes, corresponds to that which is called in France, the principal section of the crystal. For in his memoir on the Depolarization of Light by transmission, inserted in the PHILOSOPHICAL TRANSACTIONS OF THE ROYAL SOCIETY OF LONDON for 1815, page 3, he says for example: "In Mica (probably that of Siberia) the neutral axes coincide with the diagonals of its primitive rhomboidal base, and the depolarizing axes are parallels to sides of these same faces." I have translated these indications by denominations corresponding to those employed by us.

E.

primitive form of mineral salt a cube, for that of fluor spar and of alum a regular octohedron, and for that of the apophyllite a regular prism with square base. For not being able to attribute any other form to them without violating the most constant rules of crystallography, they were constrained to suppose, that simple refraction is in the three first named substances always and uniquely observed; and in the fourth double refraction at a single axes directed, following the constituent axes of the prism, properties very different from those which the celebrated Scotch philosopher had just discovered.

In fine, in the PHILOSOPHICAL TRANSACTIONS OF EDINBURGH, for 1824, page 187, Dr. Brewster publishes a remarkable memoir on *analcyne*, in which he announced that this mineral of which the integrant molecule was considered as cubical, not only acted on polarized light, but produced even a double refraction, real and observable on the natural light, which traversed it, following certain directions which he assigned. This latter phenomenon appeared then again to overturn more decisively and more evidently, the relations until that time admitted, between the forms of elementary solids, and the existence of double refraction; for the principles of crystallography absolutely refused to attribute to them as dissymmetrical configuration in analcyme. Nevertheless, in studying the work of Dr. Brewster, crystallographers were enabled to remark that he assigned to this double refraction of analcyme particular characters of developement, which distinguished it from the habitual molecular double refraction at one or two axes, inasmuch as it did not exercise it equally, as these do around one or two points, taken in each point of the crystal following parallel directions between them, but that it seemed to be referred to certain planes, fixed and locally determined in the total mass, a distinction which Dr. Brewster himself has judiciously signalized. It remained then to examine whether such species of phenomena, belonging to the entire crystal as a mass, were really allied to the configuration of elementary solids; or if they did not proceed rather from some physical action resulting from a mode of aggregation, regular also but not individual, which seemed to be established among the complex assemblages of these same solids, following a fixed but diverse construction in the different parts of the whole crystal. It did not appear that mineralogists had entertained this thought; and if they had the excessive rarity of crystals of analcyme regular and limpid, joined to their habitual diminutiveness, would not permit them to follow the developement by experiment, with a certainty suffi-

cient to convince them of the fact. Under these circumstances then it became necessary, that we should wait until some happy chance should show us the existence of a similar action, or one analogous, in crystals of a nature sufficiently common, and of a volume sufficiently considerable, in order that we might prove with facility the following laws which it exercises by cutting them, and observing them in every possible direction. These conditions were presented in unison in ammoniacal alum. Aided by this timely succour, I hope to be enabled to dissipate the doubts of crystallographers for the diverse cases which I have above pointed out; and to re-establish the reality of the relations which they had admitted between the primitive form, and the existence of the molecular double refraction, such as I have defined it. But I shall not accomplish this task without previously rendering a complete homage to the beautiful work of Dr. Brewster on the same subject. I do not partake of the sentiments of those light or jealous spirits who, when time has unveiled a new phenomena, or when more general conceptions have replaced indications which had at first appeared sufficient, amuse themselves with lessening the services of the first inventors, by raising the edifice of their disdain on that progress even, to which very often they themselves have in no wise contributed. It is much easier now, however, to analyze and class the phenomena of polarization, or of double refraction, than was the case twenty or five and twenty years ago, when Dr Brewster published the different memoirs, to the results of which I have above referred. We know at the present day exactly the laws. and the distinctive characters of two sorts of double refraction, of one or two axes, to which we may give the appellation of *moleculaire*, because they are exercised with an equal energy and in the same constructions, by the smallest sensible aggregations of those elementary solids which compose the crystallized bodies endowed with this power. We know how they develop the phenomena of colourization in polarized light; by what periodical intermitencies they act, and what degree of polarization, apparent or real, their action imprints on transmitted rays. It has been acknowledged that a simple compression or expansion artificially produced, in bodies, crystallized or not crystallized, may by these means develop an accidental double refraction, following the course foreseen. We know that several other causes, for example simple refraction associated with total and perhaps partial reflexion, develop in polarized light intermittent effects which modify the colours of thin plates endowed with the double molecular refraction. It has also been

found that the natural superposition of crystalline plates, imprints on the polarized light which traverses their intersices intermitting properties, whence results the phenomena of colourization analogous to the former ones, and fit to modify them by their association. Probably many other circumstances which are yet unknown to us, may determine similar effects, at least to our senses; and we shall only know how to distinguish them by their proper laws. But already that portion of these laws which we do possess, being applied to results anterior to their discovery, will serve with utility to analyze those complex causes which have concurred in their production. I hope they will suffice from this time, to establish between the molecular refractions, double or simple, and the primitive forms of crystals, those beautiful relations which were from the first admitted; and which the ulterior observation of complex phenomena have caused to appear invalidated. I go then with this intention,—to resume successively the study of crystalized bodies, in which these phenomena are produced.

*M. Dufrénoy at the conclusion of his last lecture made the following observations.*

The Memoir of M. Biot has caused to disappear an anomaly signalized by M. Brewster in the laws which exist between the crystallized form of minerals and their optical properties. But besides this powerful interest, it has presented us with another, that is in having recalled our attention to the great works of Haüy. M. Biot has only been too timid in the justice which he has rendered to the founder of crystallography; he seems to think, as we are pleased to repeat it during a period of fifteen years, that there exists two systems of crystallography—that of Haüy and that of the German mineralogists: the word of M. Biot is relieved with an authority so just that I think it will be of utility to relieve him from the feeling that his assertion has been too absolute. The crystallographic principles set forth by Haüy not only subsist always, but they are still almost the only ones which serve as the basis of the science which he has created; since his works, there have been added some interesting considerations on the study of crystals, but his ideas have been constantly adopted; modified sometimes it is true, either by the manner of presenting them, or of the denominations which he had admitted. In fact, for *six primitive forms*, from whence are derived all crystals, natural or artificial, have been substituted *six crystalline types*, which correspond with them exactly.

The law of the symmetry of crystals which consists in the identical position of the analogous faces has been entirely preserved.

The relation of the secondary faces, and the primitive ones, or of crystalline types, a relation which has a certain analogy with the laws which preside in the combinations of oxygen and of bases, is still almost always true. Some persons no longer admit the consideration of decreasings, that is to say the formation of secondary forms, by the application of successive plates composed of minute elementary crystals analogous to the primitive form. This ingenious idea perhaps renders still more reasonable the unknown phenomena of crystallization than any other; but besides that, it served Haüy with the means of determining the position of the faces with relation the one to the other. The German mineralogist, in place of defining the faces by this method, indicates the points when they cut the axes of the crystals; it is this consideration of axes especially, that is regarded as new, it is there, according to many persons, that the principle difference between the system of Haüy and that of the German mineralogists lies. But we deceive ourselves on this point. Haüy in many circumstances, especially for all the *octahedral forms*, refers the crystals to their axes: and for the other forms if he did not employ the same word axes, these important lines entered into the compositions of his figures. Thus, in order to calculate the law of the diminishing of a crystal, he constructed a *triangle measurer*, given by a cutting plane, passing by the axes of this crystal and taken perpendicularly to the intersection of the primitive and secondary faces of which he wished to determine the law of deviation. This triangle had for its sides: 1st. The intersection of the primitive face and of the cutting plane; 2nd. The intersection of this same plane and of the secondary face; 3rd. The axes of the crystal. The resolution of this triangle gave Haüy the number of rows or ranges taken away, and he said; the force A grown on P. by a diminution of new ranges in height on N. in breadth. In making use of axes for the same purpose, we say the face A. cuts in length M. N.: we see an almost perfect identity. Thus the method of calculating, it is true is changed: Haüy principally made use of geometry; many mineralogists make use of spherical trigonometry. M. Naumann employs the applied analysis. These different methods of calculation, may be more commodious, but they do not constitute a difference in the principles of crystallography; for the remainder we cannot yet designate them under the

name of German methods: Malus employs spherical trigonometry, and it is to M. Lamé that the first application of the equation of lines and planes to the crystallographic calculations is due.\*

The personal relations which I have had the honour of holding with several of the most celebrated mineralogists of Germany, permit me to say that they have never pretended to overturn or change the crystallographic system of Haüy; they have only added some considerations to it to make it more complete. For the rest, the determination of a great number of new minerals, the separation of feld spar into several species the application of the atomic calculation to the composition of minerals, theories of dimorphism and of isomorphism, are sufficiently beautiful titles, for which they have nothing to envy us of. I believe then that we are bound to insist on the works of Haüy, and to say that the principles which he has put forth, are still at the present day those which serve for the basis of crystallographic mineralogy.

M. Biot, thanked M. Dufrénoy for the observations which he had just made, and he would be glad to see them inserted in the *comptes rendu*. He only prayed M. Dufrénoy to bear well in mind that their positions, were very different, my experiments, said M. Biot, having for their aim to re-establish the primitive forms, and the molecular refraction double or simple, a relation which had appeared invalidated, I have been under the necessity of referring to the employment which Haüy assigned to these forms in the construction of crystals: and I have not been able to accomplish it without mentioning also the German doctrine, which avoids supporting itself on this construction, I have endeavoured to characterise, with as little inexactness as has been possible to me, the march of the two methods: but it did not belong to me to judge of them, in the manner that M. Dufrénoy can do it. If, as I believe it to comprehend, it unites the second method to the first, and to be derived from it, this connexion will give more security both to one and the other, at the same time that it will render justice to the primary inventor, who is at present but too much depreciated. But I neither have science nor the necessary authority to establish these relations, and to fix their laws.

\* On a new method of calculating crystals; by M. Lamé, Engineer of Mines (Annales des Mines, tome 4, page 64, année 1819.



*Description of a New Electro-Magnetic Coil Machine, By  
George H. Stripe, Esq. in a letter to the Editor.*

Liverpool, July 10th. 1841.

Dear Sir,

I have lately been occupied, in my leisure hours, in constructing Electro-Magnetic machines, and having succeeded as I fancy, in making one of a rather more elegant form than any other I have yet met with, I beg leave to hand you the annexed sketch and description of it, which should you consider worthy a place in your useful Journal, I shall esteem it as a favour.

I am, Sir,

Yours truly,

GEORGE H. STRIPE.

A fig. 1 Pl. VII. is a hollow pedestal  $5\frac{1}{2}$  inches across the top, and 4 inches in high, having a sunk pannel on each side: *b. b.* are the ends of a steel horse shoe magnet: *c*, the revolving electro-magnet wound round with twelve feet of covered copper wire 1-16th. of an inch diameter, the ends of the wire dipping as usual into a double trough of mercury, *d.* is a vase, serving to carry the revolving magnet; the upper part is formed into a double cup for the mercury.

The coil is fixed in the inside of the pedestal and is entirely hidden from the view. The primary wire is 110 feet long, and 1-16th. of an inch diameter; one end is soldered to the binding screw *e*, and the other passes up to one half of the mercury trough, another wire descends from the other part of the trough, and is soldered to a binding screw (not seen in the figure) on the opposite side of the pedestal corresponding in situation to *e*: these screws are for the purpose of making the connections with the battery. The secondary wire is 1-100th. of an inch in diameter, and 2400 feet long, the two ends are connected to the binding screw *f*, and a corresponding one on the opposite side.

The core of soft iron wire is fastened to the centre part of the pannel, and may be drawn out of the coil by the handle *h*, to diminish the intensity of the shock when required.

The end of the wire on the revolving magnet are hammered flat, so as to present a very thin resisting edge to the mercury in which they revolve; in addition to this a loose cap *g*, fits into the top of the cup, leaving a narrow annular space for the flattened ends of the wire to revolve in. By the adoption of these means one serious objection to the employment of mercury is overcome, by preventing the waste and uncleanness occasioned by the ends of the wire, as usually made, scattering the mercury about, when in rapid motion.

The battery I have generally made use of in connexion with the machine has only  $7\frac{1}{2}$  square inches of surface of zinc and about 10 inches of copper surface, yet small as this battery is, it is sufficient to induce a current that will fasten the hands: with a battery contained in a pint jar it will fasten the hands when the soft wire core is taken out.

At *s* is a small milled headed screw passing through a piece of brass in connexion with the binding screw *e*; opposite to the point of this screw is a wire soldered to that end of the primary coil which is in connection with the mercury cup. When the screw *s* is turned so as to cause the point to touch the wire, a shorter channel is formed for the battery current, and the coil is thrown out of action. The current now, not being reduced by having to traverse a great length of wire, causes the electro-magnet to rotate with a most rapid velocity, producing a loud humming noise. Calculating by the pitch of the note thus produced it appears that the magnet revolves upwards of 3000 times per minute.

*Description of a Cylindrical Electrical Machine with two Rubbers. By J. Goodman, Esq. M. R. C. S. &c., in a letter to the Editor.*

My Dear Sir,

In your last number of the "ANNALS OF ELECTRICITY," I observe Mr. Eaton, of Coventry, has described a double-exciting Cylindrical Electrical machine, which it appears he constructed about ten years ago.

The machine of which Mr. Eaton certainly does present a *principle* of increased excitation, well worthy of imitation, and one which must eventually be adopted by Electricians for producing a *greater amount* of fluid, at almost the same cost, and

with very little increase in dimensions; and although I by no means desire to deprive him of the originality of his contrivance, still, as the public are to be the judges, approvers, and imitators of this new principle, I cannot allow the present opportunity to pass without forwarding to you a drawing of the machine, which I have once used in your presence at the Victoria Galllery,\* and which has been several times exhibited before a public audience since the year 1828 or 1829.

This machine, constructed by myself about fourteen years ago, as you are aware possesses two rubbers, as in the machine described by Mr. Eaton; the cylinder is 14 inches in diameter by 12 inches of excitable surface, and 19 inches long between the extremities of the axis. It has *no* metallic axis passing through its centre (which latter I believe to be at all times a decided disadvantage) but is suspended upon two pivots, projecting from two brass caps. At one end (to which the winch is attached) it is supported upon an ordinary wooden pillar, as seen in plate V. fig. 1; and at the other the pivot is inserted into the centre of a *perpendicular conductor*, 16 inches long, by 3 inches in diameter, which conductor is well *insulated and supported by a glass rod* 7 inches high, from the base of the machine, and answers the purpose of a second wooden pillar, to support this extremity of the cylinder. There are two moderately thick brass arms proceeding from the upper and lower portion of the upright conductor, passing parallel to, and above, and below the cylinder, from which a number of points project to receive the fluid accumulated by excitation of the rubbers and brought round by the rotation of the cylinder. To prevent dissipation of the fluid from the extremities of the arms, each is made to terminate in a *lacquered glass ball*. The fluid is excited by means (as I before stated) of two cushions attached to the two negative conductors, in the manner described by Mr. Eaton, and the negative conductors are supported by two *glass rods*, and steel springs to accommodate the cushions to the slight unevenness of the cylinder, and the negative conductors communicate with each other by means of a brass tube twice bent at right angles, rising several inches above the upper arm of the positive conductor, and affording facility for the exhibition of their conjoined negative effects and preventing the necessity of applying more than one chain in communication with the ground, during the exhibition of the positive fluid. I may take this opportunity of corroborating Mr. Eaton's statement by saying, that I believe double amount

\* We have seen Mr. Goodman's Machine with two rubbers, on several occasions. EDIT.

of fluid is, by means of the two cushions, eliminated, to what is obtained when one only is used, and that with this magnitude of cylinder (the ends being lacquered) a sufficient surface for due insulation from rubber to rubber and in all other directions is afforded, and that no loss of fluid more than by single excitation does take place.

As a testimonial of the strength and endurance of my machine I may state that (as I believe you are aware) it has been subjected to the rapid turning of a high pressure steam engine for several days together, and in 1839, remained at the mercy, and for the perpetual amusement, of the visitors of the Mechanics' Royal Institution Exhibition here, for 3 or 4 months, and with slight repairs is now in excellent condition.

Now Sir on inspection of Mr. Eaton's machine I must confess some decided disadvantages appear which I think your readers will agree are all obviated in the machine I have just described.

The principal objection to his, appears to be the retaining of a *second wooden pillar*, to support the end of the cylinder where it is desirable the prime-conductor should be situated; and this defect is the cause of displacement of all the parts which ought to be in the situations adapted to the duty which each has to perform, and destroys all regularity, simplicity, and efficiency in the instrument.

By this arrangement, an unsightly conductor is rendered necessary at the summit of the wood pillar, and by the necessity of insulation it is obliged to be unmechanically placed some height above the level of the machine; on this account also the arms of No. 1 wire, for the purpose of collecting the fluid, are displaced, and thrown into a situation half way between the pillar and one negative conductor; and if we may estimate the distance between these two latter bodies by the half of the diameter of the cylinder, *i. e.*  $6\frac{1}{2}$  inches, at the same time allowing, and deducting only 1 inch for half the breadth of the wooden pillar, occupying the centre of the space by 12, and the two negative conductors, we shall have a space of little more than  $5\frac{1}{2}$  inches between them, through which the No. 1 wire proceeds from the upper to the lower surface of the cylinder; and consequently, the positive fluid will have (independently of its great tendency to pass from a wire of such diminished magnitude) only to escape to the negative conductor, a distance of  $2\frac{3}{4}$  inches, (and if this calculation be correct) no spark of  $2\frac{3}{4}$  inches could, by any possibility be obtained, and the dissipation of the fluid through so contracted a space, must take place continually to a great extent.

The manner by which the arms and forks are suspended, appears to me to be un-mechanical, and very liable to displacement. The elevated position of the positive conductor, would prevent the arrangement of conducting communications between the negative conductors above the Cylinder, which is the only situation in which this can be planed—if a proper distance from other bodies must be allowed.

I think, Sir, by comparing these defects with the description and drawing of my machine, you will find that in it, they are all obviated. I also present you with a drawing of a Plate Machine, which I have arranged on the same principle, which if deemed worthy a Lithograph in your valuable Periodical, I shall feel gratified.

It appears to me to possess greater simplicity, and fewer number of parts—for the same action and effects—than any I have yet seen. The only difference between the arrangement of this and the Cylinder-Machine, is the adoption of a laquered glass insulating axis, cemented into brass caps, as in the drawing, which will undoubtedly render the insulation of the plate more complete. The nature of the parts and their arrangement, will I think, be readily seen and understood by an inspection of fig. 2. plate V.

I am, Sir,

Yours Respectfully,

JOHN GOODMAN.

GREENGATE, SALFORD, July 19th. 1841.

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P. S.—I find that for common purposes, and where extreme cheapness is desirable, the plate of the latter machine may be made of common window-glass, to the centre of which two wood-turned convex caps may be cemented without any perforation of the plate, and the axle is completed by cementing a glass rod into the centre of each cap.

The cement I use is equal parts of rosin and bees'-wax, made sufficiently thick by the addition of red ochre. The cost of a plate of 15 ins. diameter, is about two Shillings or Half-a-Crown.

CONVERSAZIONI AT THE ROYAL VICTORIA GALLERY OF  
PRACTICAL SCIENCE, MANCHESTER.*On the Combustion of Coal and the Generation of Smoke.**By Charles Wye Williams, Esq. of Liverpool. \**

(Continued from page 156.)

There is no part of the operations connected with a furnace in which practice is more at fault, and in which greater chemical and practical blunders are committed, than in what is familiarly termed "*Smoke burning*". This is wholly attributable to a neglect of the Chemistry of Combustion: and hence we are led, both to the use of incorrect terms and the adoption of an unsound practice which only leads us further astray. As far as the use of fuel is concerned, practical men must take chemistry as their guide: they must search into its processes and conform to its laws, for all that belongs to combustion and the generation of heat, is purely chemical: the mechanical construction of boilers and furnaces with their varied forms, proportions, and relations, are worthy of attention as they minister to the wants and processes of nature and as indicated by the lights of Chemical science.

In a letter lately published by a practical Engineer, objections were taken to the plan of introducing air to the gases in a furnace, behind the bridge; and these objections were justified on the ground that, "*all sensible operative Engineers have the greatest dislike to it.*" This is indeed shutting the door against improvement while this writer seems unconscious

\* We have had great pleasure in witnessing the beneficial effects of Mr. Williams's plan of constructing furnaces for the prevention of smoke, at the works of Messrs. John and Joseph Lockett, Engravers, Manchester. Mr. Williams has contrived to place windows in the sides of his furnace, through which an observer can see what is going on within; and by means of an iron door, the draught-hole, which admits air to his diffusing filtre, can be closed at pleasure, and the furnace allowed to work in the usual way. When this door is closed, the space beyond the bridge becomes immediately filled with the blackest smoke, completely impenetrable to the eye, and volumes of it roll from the top of the chimney into the atmosphere above; but as soon as the door is re-opened, and the diffusion-apparatus brought into operation, the smoke vanishes entirely, and a bright flame fills the space which it before occupied beneath the boiler; and the dense black clouds which had issued from the chimney, vanish, as if by magic, and leave the atmosphere through which they had rolled as clear as if no fire were left in the furnace below. Indeed, so very prompt and decisive are the beneficial effects of Mr. Williams's plan, illustrated by the furnace at Mr. Lockett's works, that the mere opening or closing of the iron door above mentioned, even when new coals have just been thrown on to the fire, is sufficient to convince the most scrupulous observer. EDIT,

of the fact, that these "sensible operatives" were the very parties who required to be set right, and taken out of the hands of those who have hitherto been leading them from one error to error.

But why should boiler makers or furnace builders be opposed to the suggestions derived from chemical experience more than other classes of practical men. We see what advances have been made by the aid of chemistry in the arts of Printing and Dying: the processes of which, as Dr. Ure observes, "were once supposed to depend on mechanical laws. Why then should mechanical Engineers object to being shown the principles on which the construction of furnaces for evaporative purposes may be made to harmonize with chemical processes, any more than those for the manufacturing of Acids or Alkalies? The laws which govern the combustion of fuel and the converting it to the purposes of heat or light, belong as much to chemical science as those which regulate the use of mordants in dying.

There is nothing more remarkable than the fact that the interference and suggestions of scientific improvements of our furnaces and boilers, are regarded with great jealousy by the class of operative Engineers, who regulate these matters in the manufacturing districts. There is nothing more common than to find that while the greatest attention is paid to the sizes and relative proportions of the several parts of furnaces and boilers, there is the greatest neglect existing in regard to the effect of these details, or whether they favour or obstruct those processes without which combustion cannot take place. It would seem indeed as if our practical men and mechanical Engineers imagined there was some fixed, though hitherto unknown law or standard of relative proportions, mechanically considered, between the several parts of a boiler with its furnace and flues; and on which the perfections of the whole process of combustion and evaporation depended, and that to discover this standard was the great object of their search: as though the whole question resolved itself into one of mechanical proportions rather than mechanical conditions.

But what should we say, if one who undertaking a difficult complex chemical process, should pay great attention to the size and cubical contents of his retort, and the diameter and length of its beak; with the proportions these bore to the area of the furnace or bath on which it was heated: and taking credit for a careful observation of these merely mechanical details, should overlook the relative quantities of his ingredients—the successive states of solid, liquid, or gases:—the

several changes going on in his retort—the nature of his products, the quantity that might escape, or the degree of heat required for their development or combination. What should we say of this operator if neglecting such chemical details, he should nevertheless effect to give accurate results and draw correct chemical inferences: and rejecting the aid of an experienced chemist, characterize his suggestions as mere theories, or Scientific niceties unworthy of attention, when opposed to his own experience as “*a practical man*”: and gravely assure us, that all sensible operative Engineers have the greatest dislike to them.

Yet such is the course with many *soi-distant* mechanical Engineers, although with all their practical skill, no settled plan or principle has yet been decided on for the construction of even ordinary boilers and furnaces, at least so as to economize fuel.

No two of them in fact agree: all continues to be uncertainty and experiment: whereas we find among Scientific men more system in the details of the most complicated chemical processes, and the description of the whole in what they are carried on, than we do among “practical Operative Engineers” in managing the combustion in the ordinary operation of the combustion of fuel, and the construction of the furnaces in which it takes place.

Now these observations apply directly to the case before us, namely, the adoption of Mechanical Contrivances for the “combustion of Smoke,” rather than chemical aids towards its prevention.

It is alledged that “smoke burning” is a familiar and well-understood phrase: “that it is familiar” will not be questioned, but that it is “understood” is assuredly not the case, and a little enquiry will convince us of the fact.

It will be conceded, that we are not justified in applying the term “*smoke*,” to the *gas* we burn in our lamps: neither can we apply it to carbonic acid, nor the steam—the two products of the combustion of that gas. One half the oxygen required for such combustion going to produce the former, and the other half the latter. Yet these are the principal substances to which, as they issue from our furnaces and chimney shafts, the term is applied.

The question thus remains to be answered. What is Smoke? And how are we to distinguish between its combustible and incombustible properties? These questions lead us directly to the conclusion, that though it may not be practicable to effect the combustion of Smoke, it is nevertheless practicable



to effect the combustion of the whole of the fuel from which it issues, and thus prevent its formation: yet this distinction is little attended to or understood in practice.

In a paper inserted in a late periodical the author suggests a modification of Jeffrey's plan for carrying off the solid matter of Smoke by the aid of water, in the way of a Shower Bath, introduced into the Chimney Shaft; where mingling with such solid matter it carries it away into the sewer.

But without enquiring into the merit of the suggestion, these are the previous questions which should first be answered, namely: why permit this smoke to be generated? why carry on the process of combustion so imperfectly as to suffer any of the combustible ingredients to escape? or why incur, not only the loss of these ingredients, but the trouble and expense of neutralizing their effects, and preventing a nuisance which we have thus created by our own slovenly operations.

Had chemistry been consulted instead of mechanics, this ingenuity in contriving a *mechanical* remedy would have been superceeded by a *chemical* one: and if scientific men have hitherto been unsuccessful in effecting perfect combustion of fuel in all furnaces, that can be no reason for discarding their services and recurring to practical mechanics, who are certainly less likely to discover a remedy for this chemical nuisance. Their want of success hitherto should have operated rather as a stimulous for further investigation than an abandonment of the service to less scientific hands.

To distinguish between what is, and what is not smoke, constitutes the first step in the enquiry—When we see a black cloudy matter issuing from an ill-adjusted *lamp*, we properly call it *smoke*; and were we to confine the term to the same substance in the *furnace*, we should avoid those practical errors into which a loose mode of expression has led us.

In the last number of the "ANNALS OF ELECTRICITY," was given an analysis of those products which escape from a chimney shaft. It was there shown that smoke was but one, out of many of these products; and that being the result of an imperfectly managed process, it might, under a more perfect one, have been avoided.

The real question then is, how should the chemical process of combustion be conducted so as to avoid the loss of any of the constituents of the fuel, and the formation or existence of smoke? From the flame of a well-adjusted gas-burner, there is nothing escapes that is combustible; if it be otherwise in the furnace where the same gas is burned, there must be such a difference in the process as to demand our attention and correction.

In the paper in the "Annals," referred to, it was shown that the aggregate of what escaped from the chimney shaft was, 1st, Steam, 2nd. Carbonic Acid, 3rd. Carbonic Oxide, 4th. Smoke; under the erroneous impressions that this aggregate of matter was combustible, many ingenious men have taken out patents for effecting its combustion,

Without going into detail it may be stated that the leading principle which seems to have governed most of these "smoke burning" patentees, was, the bringing the mass of products, as they arise in the furnace, into contact with a body of ignited or incandescent fuel, and by which it is alledged, it will be burned. In this way, almost *ipsissimis verbis*, have these inventors been describing their operations, from the days of Watt down to our own: for these also are the words of Watt in his patent of 1785.

One patentee observes that "the gases issuing from the body of fresh coals in one set of bars, pass *over and among* the body of glowing coals on a second set of bars, by which they would be consumed." Another says: "The direction of the smoke and gases is repulsed by dampers, while the fresh fuel is introducing and the smoke emitted is drawn over the red-hot fuel of the next fire place, by which the smoke becomes consumed." A third patentee writes, that the object of his invention is the "causing the smoke to come in contact with the charged, or red-hot coal, and as it rises from the green or fresh coal on one set of bars in the front part of the furnace, it will be perfectly consumed in passing over the second, or red-hot fire, instead of being allowed to escape with the gaseous products up the chimney, yet in all these descriptions not a word is said respecting the really essential part, the bringing these gases into contact with the air. Now whether we consider these products in the mass, as they arise in the furnaces, or their component parts separately, these patentees are chemically in error in either view of the subject. They seek to effect combustion by means of a high temperature, yet it is not temperature, nor contact with "red-hot fuel" which can produce this result. It is contact with atmospheric air, in *due proportion, in the proper place, and in the proper manner that is required.*

There is in fact no degree of heat—even the very highest, that can effect combustion: as heat is but one of the incidents or conditions of that Chemical and Electrical union—which is combustion.

If an atom of hydrogen or carbon can drive out heat, by chemically combining with a given number of atoms of oxygen,

it is manifest that our object should be, not the raising such hydrogen or carbon to any given high temperature; but the providing the due proportion or equivalent of this oxygen, and bringing them within the range of mutual action.

If this be effected, they will combine at the temperature of the furnace, which is always sufficient for the purpose, without the aid of ignited fuel. But if the oxygen be not applied in the proper mode and quantity, all the heat of the Universe will not effect its combustion.

A due consideration of these essentials will shew the fallacy of such "smoke burning" inventions. These patentees do not distinguish between the heat required for the first ignition, or lighting the gas, and the continuation of such ignition in the form of flame: an examination, however, of the distinction between the chemical conditions of combustion, and the temperature required to the ignition of gaseous matter, would shew that such high temperature was not only unnecessary, but even might be injurious.

The consideration, however, of this part of the subject, must be deferred to a further opportunity.

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*Note on an Aerolite, which fell on the 12th. of June, in the environs of Chateau Renard. Letter from M. Delavaux, to M. Warden.\**

From the interest which I know you take in every thing which belongs to the Sciences, I think you will be pleased by my informing you of a phenomenon, which has thrown the whole population of this city and its environs, into a state of wonder and amazement. On Saturday last, the 12th. of the current month, between one and two hours after mid-day, with clear and serene weather, and a temperature of 17 or 18 degrees of the centrigadal Thermometer, the sky presenting the appearance of clouds, this place and its environs were alarmed by an explosion far louder than could have been produced by the discharge of one of the largest pieces of artillery. No person could any way account for this extraordinary explosion, on which were made a thousand versions more or less ridiculous. I conjectured that it could only have been pro-

\* Comptes Rendus.

duced by the fall of an *Aerolite*; and yesterday, in my excursions abroad, I was convinced of the truth of my conjectures. After several indications made in the country, I went to the field of the Bourgonniera, between the farms of *Thezars* and *Petits-Marleaux*, near the *Ru de Fillargis*, commune of Frigures. There I observed on the ground, at that place very stony hard, two circular and hemispherical pits or hollows, distant and from each other about thirty paces, the largest of which was 35 centimetres in breadth, the other only 30: and presenting a depth of 14 or 15 centimetres. They were still encircled by a great number of small fragments of aerolites. I collected instantly about fifty of them. The most important thing was to procure the most voluminous and incontestable pieces; I found one very fine one at the house of a person named Pycrs, a labourer on the farm of Petits- Marteaux, and I made acquisition of it.

This piece weighs more than 3 killogrames, and it is not one half of the *Aerolite* of which it formed a part, as may easily be judged from its form. Francis Bertrand, another cultivator on the farm of Grunde Marteaux, has another fragment weighing little less than the former. But the largest of these *Aerolites*, though it is also broken, is that now in the possession of M Cendre, a rich proprietor of the commune of Triguères. The principal fragment, which I weighed with a steelyard has a weight of more than 15 killogrames. M. Cendre has another fragment weighing about 1 killogramme, another proceeding from the same, and of about a killogramme in weight, is in the hands of the Petit Mayor of the commune, and a distinguished Advocate at the bar of Montargis. There are also many fragments in the hands of the neighbouring villagers.

M. Cordier, on the reading of this note, announced that the Museum of Natural History, has already taken measures to procure indications on the circumstances which accompanied the fall of this *Aerolite*, and likewise to obtain fragments of it.

M. Arago recollected that the Journals spoke, some months ago, of the fall of another *Aerolite*, which had been observed in the neighbourhood of Beaune, and wished to know if it could not be arranged to take proceedings on this subject, similar to those of which M. Cordier has just spoken. A person employed on the telegraph, it has been said, has collected some fragments of this meteoric stone and would probably be able to set us in the way of making ulterior researches on the subject.

*On a Meteor observed at Angers, on the 9th. of June, 1841, Letter from M. Morren to M. Arago.\**

I have read in several Journals, that a very remarkable Meteor had been seen in different localities on the 9th. of June, about 8 o'clock in the Evening. • I think then that I am bound to send you some particulars on the same object, persuaded, that if besides them, you receive some precise details, it will be easy to establish the identity of the Meteor perceived at the same hour in different places; and to calculate almost exactly the height at which it moved above the surface of the earth.

Its march was not a rapid one, thus it hath been said it had the appearance of a star of large magnitude, leaving after it a luminous train; to all the observers who saw it from Algiers, it moved in the direction from East to West, in the southern part of the Heavens. As this Meteor, the course of which seemed parallel to the horizon, seemed (to some observers) to verge towards the summit of an avenue of trees, in a direction almost similar to its own. I was enabled to measure with sufficient exactitude its height above the horizon, from the projectory point, which seemed to me to be the nearest point to the observer.

This height is comprised between 46 and 47° (nonagesimal division). It preserved the same sensible height during the whole period of its progress, the speed of which appeared to be regular, and was unaccompanied by any noise.

M. Ledoux, Physician to the Thermal waters of Baynoles, (Orne) addressed at the same time to M. Arago, some indications made on the same Meteor, observed at Baynoles and in a neighbouring village.—COMPTES RENDUS, No. 25, 21st. JUNE, 1841.

*On the Preparation of Formic Ether, by Wöhler, (Annalen der Chemie und Pharmacie, Vol. 35, col. 2, P. 238.)*

The following process gives formed ether in great quantity and with the greatest facility. We pour into a distilling vessel on an intimate mixture of 10 parts of starch and 37 parts of the peroxide of manganese in very fine powder, another mixture of 30 parts sulphuric acid, 15 parts of water,

\* Comptes Rendus.

and 15 parts of spirits of wine perfectly rectified: by the aid of some coals we raise it to a light ebullition; we then distil it until the product of the distillation no longer contains ether. In order to separate the water from the alcohol, we cause a sufficient quantity of Chloride of Calcium to be dissolved, and we then distil the ether in a *balneum mariæ*: we accomplish its purification in again treating it after the same manner.

Each of the experiments which M. Koble has made at my instigation, on this subject, has presented a phenomenon which perhaps indicates the presence of some particular body in this ether, and the explication of which will exact some new researches. In fact, when the fragments of the chloride of calcium thrown down, have been during some time in contact with the ether which has already undergone a primary rectification and is by it dissolved in part, it becomes of a decided yellow colour and leaves deposited on the sides of the vessel small crystals, very fine and quite colourless. This colour ordinarily disappears at the end of a short period of time. Perhaps it is due to the chloride of iron, and perhaps the crystals may be due to the combination of chloride of calcium with alcohol, which is soluble in formic ether, and can deposit itself in crystals.

A. G. Y.

*Note on the employment of bromine in photography on plates:  
by M. Fizeau.\**

Chlorine, and more especially bromine of silver, prepared by the humid method, being more susceptible of impressions from light than iodine, permits us to hope that their application to the production of photographic plates, will conduce to the surpassing of the sensibility, already so great of the reagent of M. Daguerre, it will also enable us to make a great number of experiments on this point. In Germany chloride of bromine is employed, in England the bromide of iodine, with which I on my part have made some trials; in fact, there was presented to the Academy, during its last session, the beautiful results obtained by the aid of a method recommended by M. Claudet: this time it is the chloride of iodine which has been employed, and the sensibility of the impressionable layer is sufficiently increased to reduce to two minutes the duration of the exposure in the darkened chamber.

\* Comptes Rendus.

I have myself met with a means of arriving at a sensibility still greater, by the employment of bromine: but the announcement made by M. Daguerre, of processes much more perfect had induced me to await their publication. The process of M. Claudet being somewhat approaching to mine, I think it my duty to explain the method of which I have made use.

The ordinary iodized plate is exposed for some instants to the vapour of a weak solution of bromine in water; the colour of the sensitive layer changes but very slightly under the influence of bromine, and in such a manner that it requires a little practice and attention to appreciate the time necessary for this operation.

The plates thus bromized, then possess a high degree of sensitiveness, and the duration of the exposure in the darkened chamber is reduced to the third part of a minute. I speak here of the darkened chamber of M. Daguerre, to which it is important in all cases to refer to, in order that the results may be comparable. In fact, the rapidity of the operation depends on the intensity of the light; and the intensity of the light at the centre of a lens being given by the relation  $i = \frac{r^2}{d^2}$   $r$  being the opening ray and  $d$  the focal distance, we see that in varying these two quantities, we may vary the intensity at pleasure. It is true that two causes, the reduction of the image and the aberration, prevent the augmentation of this intensity indefinitely; but still we have been able, by the aid of simple modifications of construction to make it vary sufficiently, to reduce the duration of the exposure in the dark chamber to one or two minutes with the ordinary iodized plate.

*Formation of Sulphate of Manganese in the spontaneously inflammable Coals: by F. Mohr, (Annalen der Chemie and Pharmasie Vol. 35, col. 2, p. 239),\**

In December 1839, after a prolonged period of rain, there occurred on the quay of the Moselle, a spontaneous inflammation of a large mass of small pit coal; at first watery vapours were seen to disengage themselves; these soon became still thicker, and the odour of coal and rock-oil in combustion were not tardy in spreading themselves: it was not until after six weeks duration of these phenomena that any attempt was

made to extinguish the combustion. There was found amongst the fragments of half consumed coal, which had become agglomerated into a mass, a saline substance of a clear yellow, colour, and which could easily be separated from the coal. It was soluble in water and had a bitter and acerbous taste. It gave with the salts of baryta a precipitate insoluble in the acids. Heated on a plate of platinum it puffed up, and gave by combustion a clear white residue, with disengagement of sulphuric acid gas. This residue did not effervesce with the acids; with the carbonate of ammonia, there was no precipitate; with caustic ammonia, there was an abundant precipitate, soluble in chlorhydrate of ammonia; with the basic phosphate of ammonia it gave an abundant crystalline precipitate; with caustic potash, a white precipitate; with tartrate of ammonia, there was no reaction. We see by these results that the salt in question was a sulphate of manganese. The greater part of the sample was then treated with water; the liquid was filtered, then evaporated, and we obtained a great quantity of sulphate of manganese crystallized. There can be no doubt as to the cause of the production of this salt. The cinders of coals contained manganese, and the sulphurous pyrites in combustion furnished the sulphurous acid, which is by degrees transformed into sulphuric acid by the contact of the atmospheric oxygen, and is at the same time deprived of manganese. It is in similar slow and continued chemical reactions, that the most curious phenomena are often produced, such as the formation observed some time ago, of the cyanuret of potassium in the ridges of furnaces. A. G. Y.

*On the appearance of Lightning, in a letter to the Editor.*

Manchester, August 23rd. 1841.

SIR,

On the 17th. of September, 1838,\* I addressed a Letter to you concerning a phenomenon, which, though very striking and of great importance to Electrical Theory, had not attracted much of the attention of Scientific men.

I observed, that Lightning does not always assume the appearance of a single momentary flash, but frequently of two distinct flashes, of which one succeeds the other after an unpreciable length of time; and that whenever the Lightning of a distant Storm is observed by night, it is still seen to observe its double character. I did not venture any speculations on



the cause of the phenomenon, but left it for explanation to your more Scientific readers.

My attention, however, has been again directed to this subject by a paper of Professor Faraday's, which was inserted in the "*PHILOSOPHICAL MAGAZINE*" for August. This eminent Electrician, referring to the exhibition of distant Lightning by night, notices some appearances, which he says are due to light reflected from the edges of clouds, and attributes the sensible duration of the flash "to two or three flashes occurring very suddenly at the same place, or nearly so, and illuminating the same edge of a cloud."

Faraday seems to imagine that two or three flashes sometimes follow one another at the same place, rather from accident than from any law of nature. But it is easy to see, that, as after one flash the clouds must be re-charged before another can take place; such an event as the immediate recurrence of a flash in the same place, could, on such principles, exist only as an extraordinary and very rare phenomenon.

But the double flash is not an extraordinary, but the *common* appearance of lightning; and as far as I can judge, the component flashes are of equal intensity, the interval of time between them being about one-third of a second. I have seen a forked stream extending from 45° above the horizon to the ground, followed the next instant by another stream, in every respect exactly like the first, and appearing in the identical place: and though I have seldom had the fortune to witness such a splendid exhibition as the above, I have frequently observed the very same appearances on a smaller scale, and when the Lightning itself was seen—not the illuminated edge of a cloud.

Since the beautiful series of experiments was made by Professor Henry, on the induction of currents of common Electricity, no doubt remains on my mind that the second flash is the effect of the induction of the first: and that if a third flash be produced, (which is rare, on account of the exhaustion of the cloud by the second flash,) it is occasioned by induction of the second. In the case of sparks from Electrical apparatus, secondary effects doubtless take place, though so rapidly as to be inappreciable.

I can hardly conceive how the peculiar character of Lightning can have been unnoticed by any attentive observer of

Nature. Dr. Watts seems to have had it in his mind, when, in one of his beautiful "Divine Songs" he thus adapts the language of the 29th. Psalm :

"A sovereign voice divides the flames,  
And thunders roars along."

YOUR VERY OBEDIENT SERVANT,

P.

XIX. *On some supposed forms of Lightning.* by Michael Faraday, D. C. L., F. R. S.

The magnificent display of lightning which we had on the evening of the 27th of last month, and its peculiar appearance to crowds of observers at London, with the consequent impressions on their minds, induces me to say a few words on certain supposed appearances and forms of lightning, respecting which the judgment of even good observers is often in error.

When, after a serene sky, or one that is not overcast, thunder-clouds form in the distance, the observer sees the clouds and the illumination of the lightning displayed before him as a magnificent picture; and what he often takes to be forked lightning (i. e. the actual flash, and not a reflection of it), appears to run through the clouds in the most beautiful manner. This was the case on that evening to those who, being in London, observed the storm in the west, about nine o'clock, when the clouds were at a distance of twenty miles or more; and I have very frequently observed the same effect from our southern coasts over the sea. In many of these cases, that which is thought to be the electrical discharge is only the illuminated edge of a cloud, beyond and behind which the real discharge occurs. It is in its nature like the bright enlightened edge which a dark well-defined cloud often presents when between the sun and the observer; and even the moon also frequently produces similar appearances. In the case of its production by lightning and distant clouds, the line is so bright by comparison with the previous state of the clouds and sky, so sudden and brief in its existence, so perfectly defined, and of such a form, as to lead every one at the first moment to think it is the lightning itself which appears.

But the forms which this line assumes, being dependent on the forms of the clouds, vary much, and have led to many

\* We have considered that Dr. Faraday's remarks on lightning may possibly be interesting to some of our readers. The opinion of Count Mahon on this subject, we will give a place to in an early number. EDIT.

mistakes about the shape of the lightning flash. Often, when the lightning is supposed to be seen darting from one cloud to another, it is only this illuminated edge which the observer sees. On other occasions, when he was sure he saw it ascend, it was simply this line more brilliant at its upper than at its lower part. Some writers have described curved flashes of lightning, the electric fluid having parted from the clouds, gone obliquely downwards to the sea, and then turned upwards to the clouds again: this effect I have occasionally seen, and have always found it to be merely the illuminated edge of a cloud.

I have seen cases of this kind in which the flash appeared to divide in its course, one stream separating into two; and when flashes seen at a distance are supposed to exhibit this rare condition, it is very important the observer should be aware of this very probable cause of deception.

I have also frequently seen, and others with me, a flash having an apparently sensible duration, as if it were a momentary stream, rather than that sudden, brief flash which the electric spark always presents, whose duration even Wheatstone could not appreciate. This I attribute to two or three flashes occurring very suddenly in succession at the same place, or nearly so, and illuminating the same edge of a cloud.

The effect I have described can frequently be easily traced to its cause, and when thus traced best prepares the mind to appreciate the mistakes it may lead, and has led, to in the character, shape and condition of the lightning flash. It often happens at the sea-side, that, after a fine day, clouds will towards evening collect over the sea on the horizon, and lightning will flash about and amongst them, recurring at intervals as short as two or three seconds, for an hour or more together. At such times the observer may think he sees the lightning of a flash; but if he waits till the next illumination, or some future one, takes place, he will perceive that the flash appears a second time in the same place, and with the same form; or perhaps it has travelled a little distance to the left or right, and yet has the same form as before. Sometimes an apparent flash, having the same shape, has occurred three or four times in succession; and sometimes it has happened that a certain shaped flash having appeared in a certain place, other flashes have appeared in other places, then the first has reappeared in its place, and even the others again in their places. Now in all these cases it was simply the illuminated edges of clouds that were seen, and not the real flashes of

lightning. These forms frequently appear to be in the cloud, and yet are not distinguishable till the lightning occurs. It is easy, however, to understand why they are then only developed, for that which appears in the distance to be one dull mass of cloud, distinguishable in figure only at its principle outline, often consists of many subordinate and well-shaped masses, which, when the lightning occurs amongst or beyond them, present forms and lines before unperceived.

The apparent duration, which I before spoke of, is merely a case of very rapid recurrence, and may, by a careful observer, be easily connected with that which I have now proposed as the best test of the nature of the phenomenon.

There are some other circumstances which will help to distinguish the effect I have thus endeavoured to describe from the true appearance of the lightning flash, as the apparent thickness, sometimes, of the supposed flash, and its degree of illumination; but I have, I think, said enough to call attention to the point; and, considering how often the philosopher is, in respect to the character of these appearances, obliged to depend upon the report of casual observers, the tendency of whose minds is generally rather to give way to their surprise than to simplify what may seem remarkable, I hope I have not said too much.

June 22, 1841.

M. FARADAY.

Phil. Mag.

IX. *On the Polarization of the Chemical Rays of Light.*  
By JOHN SUTHERLAND, M. D. of Liverpool.\*

It has been long known that the invisible rays of the solar light, which manifest their presence by inducing chemical action, are possessed of some of the properties of the luminous rays. Their capacity of being reflected and refracted must have been observed at the time of their discovery; and Dr. Thomas Young proved, that they were capable of producing the phenomena of interference, by allowing the rays beyond the violet extremity of the spectrum to fall on paper covered with chloride of silver, after having been transmitted through glasses showing Newton's rings. The same phenomenon was also exhibited directly by M. Arago, who made use of Fresnel's experiment for the purpose of demonstrating it.

\* Read before the Royal Society of Edinburgh, December 21st. 1840.

On the 21st. December 1812, M. J. E. Bérard read a paper before the French Institute, "*Sur les propriétés des différentes espèces de rayons qu'on peut séparer au moyen du prisme de la lumière solaire,*" which was published in the "*Mémoires d'Arcueil*, vol. iii.; and in this memoir, after investigating several properties of the chemical rays, he relates the following experiment:—"I received the chemical rays directed into the plane of the meridian, or an unsilvered glass, under an incidence of  $35^{\circ} 6'$ . The rays reflected by the first glass were received upon a second under the same incidence. I found that when this was turned towards the south, the muriate of silver exposed to the invisible rays, which it reflected, was darkened in less than half an hour; whereas, when it was turned towards the west, the muriate of silver exposed in the place where the rays ought to have been reflected, was not darkened, although it was left exposed for two hours." From this experiment he deduces that the chemical rays can be polarized like white light, when they are reflected by surfaces of glass under a certain angle, and that this angle appears to be very nearly the same for the two kinds of rays. "It is," he says, "consequently to be presumed that the chemical rays can undergo double refraction in traversing certain diaphanous bodies; and lastly, we may say that they enjoy the same physical properties as light in general."

An experiment similar to M. Bérard's will be found detailed in the following paper, although I was not aware of there being any such on record, till informed of it by the kindness of Professor Forbes, after my paper had been read. In a communication which I have recently received from him, he also says: "In Spring 1839 I tried the experiment of letting the picture formed by polarized light passing through calcareous spar fall upon sensitive paper, then newly discovered;—whether from the fault of the paper, I know not, but on my first trial I obtained no kind of effect, and my attention being occupied with other matters, I never repeated it; but at Birmingham, in August 1839, being requested to give some account of the Daguerreotype, which I had seen in Paris, I mentioned the experiment, and pointed out its valuable application to fix with unerring accuracy phenomena of diffraction and polarization, which different eyes have seen differently, and which, regarded as the test of theories, would thus be preserved with unimpeachable fidelity for examination at leisure by every eye."

With these few preliminary remarks I shall proceed to the paper itself.

In the course of last Summer it occurred to me that the invisible chemical rays of light might be subject to the laws of polarization, and early in the month of July I instituted a series of experiments to determine the point. In all investigations of this nature it is of importance that the solar light should continue for a certain time of nearly the same intensity; but during the Summer the sky was so frequently overcast, that a very few days only could be devoted to the subject, and for a considerable part of the Autumn the Sun's altitude has been too low. For these reasons, I have been unable to pursue the investigation so far as I could have wished, but I have nevertheless obtained a sufficient number of results to establish the principal facts; and I have thought it better to bring these forward at the present time than to allow the subject to lie over for the several months which must intervene before I can again resume it.

I have succeeded in polarizing the chemical rays: 1st. as they proceed directly from the Sun; 2nd. as they exist at the extreme violet end of the spectrum: 3rd. as they fall from the sky: and by three different processes—double refraction, reflection, and repeated single refraction— I shall describe in succession these processes, with the apparatus used.

### 1. *Polarization of the Chemical Rays by double Refraction.*

The first important fact in regard to the chemical rays is, that they are susceptible of double refraction, in the same manner as the luminous rays are. To prove this, a prism of calcareous spar, one and three-quarters of an inch in length, and one inch in the side, and polished at one end was employed. It was inclosed in a case having an aperture at one extremity, the other extremity being open. The extreme violet rays of the solar spectrum were allowed to pass through the aperture and to fall on a piece of photogenic paper. Two very faint images were formed, and in a minute or two these produced corresponding dark impressions on the paper. A similar result was also obtained when the direct Sun's rays were employed. By this apparatus two impressions of equal intensity, each half an inch long and one-eighth of an inch broad were obtained: but on extending the experiments I soon found that a polarized beam of greater size than it could give was necessary; I therefore substituted a plate of Iceland spar, an inch square, and 3-16ths. of an inch thick; and in order to increase the divergence of the rays, one of the planes was ground to an angle of  $63^{\circ}$  with the obtuse edge, and both planes were then polished. A plate of Iceland spar thus prepared, has the pro-

perty of separating the two rays so much, that when inserted into an aperture admitting a sun-beam into a darkened room, it gives two images of polarized light, each one inch in diameter, and about an inch apart, on a screen placed at the distance of eight feet from the aperture.

These images, when received on sensitive paper, both produced considerable effect; but the extraordinary more than the ordinary, and it was therefore chosen for the purpose of experiment.

An analyzing apparatus, consisting of six thin plates of mica, was placed obliquely in the course of the polarized ray, so as to form with its axis an angle of about  $25^{\circ}$ . The instrument was turned round until the plane of the mica plates coincided with the plane of polarization of the ray. When this was done the light was almost extinguished, and was allowed to fall on a piece of photogenic paper. After the lapse of five minutes no effect whatever was produced on the paper. The mica plates were then turned round  $90^{\circ}$ , until their plane was at right angles with the plane of polarization. The light was greatly increased in intensity, and in one minute the paper was tinged, in three minutes a good deal so, and in five minutes it was pretty dark. This experiment proves that the plane of polarization of the chemical rays is coincident with that of the luminous rays of the sun's light.

Instead of the mica plates employed in the last experiment, I next used the long prism of Iceland spar already mentioned. The polarized beam was transmitted along it, and the prism turned on its axis until one of the rays was extinguished: a piece of sensitive paper received a dark image from the unextinguished ray; but the extinguished ray produced no effect whatever.

A film of mica was then placed in the course of the polarized beam before it passed through the prism, and the extinguished ray immediately reappeared: the two rays were allowed to fall on sensitive paper, and both produced tints of equal intensity.

This experiment was repeated with a film of selenite instead of the mica: one of the rays was coloured of a yellowish, the other of a purple tint: on being received on photogenic paper both the images gave dark impressions, but the purple image produced more effect than the other.

The experiment was again repeated with a film of selenite, which gave a pink colour to one ray and a green colour to the other; and in this instance both images gave tints of equal

depth to the paper. These experiments go to prove that the chemical rays, when polarized, are acted upon by thin crystallized plates, in a manner similar to that in which the luminous rays are influenced.

I was next desirous of ascertaining whether any phenomena resembling the coloured rings seen round the axes of crystals in polarized light were presented by the chemical rays when polarized, and for this purpose I employed an apparatus consisting of a tube two inches long and three-fourths of an inch in diameter: and at one extremity was placed a double convex lens, having a focus of one and a quarter inch: within the tube, and at the distance of half an inch from the lens, was placed a section of a calcareous spar rhomb, such as is used for showing the coloured rings, At the other extremity of the tube was placed an oblique analyzing bundle of three mica plates, or one of Nicol's improved prisms, and the apparatus was so disposed that the polarized sunbeam was allowed to fall on the lens, and thence through the tube upon a screen placed close to it. An image of the coloured rings and black cross was thus obtained, and by turning the tube  $90^\circ$  upon its axis, the rings with the white cross appeared; while in this position a piece of photogenic paper was used to receive the image and a reversed impression of the rings and cross was obtained; to wit, the place where the white cross had been was dark, the centre light, with a complete black ring round it, and segments of other rings exterior to it, The tube was next turned  $90^\circ$  upon its axis, so as to show an image of the rings with the black cross; sensitive paper was again employed, and another reversed impression obtained; to wit, the position of the black cross was white, the centre and interspaces dark, with segments of two or three darker circles on them.

I have also used a section of rock crystal for the purpose of obtaining impressions of its rings, and to determine whether phenomena were presented similar to those of circular polarization. The result, however, owing to the unsettled state of the weather, was not so satisfactory as I could have desired, and I have therefore left this part of the subject for a more favourable opportunity.

The next step in the investigation was to determine whether similar phenomena were presented at the violet extremity of the spectrum. For this purpose I employed a glass prism to decompose the polarized sunbeam used in the preceding experiments. A polarized spectrum was thus formed, at the extreme violet end of which most of the experiments were re-



peated. The extreme violet ray was allowed to pass through the prism of calcareous spar, and received on photogenic paper; an image of violet light, extremely feeble, was all that was visible. The action of the chemical ray was, however, intense, for in a minute or two a deep dark spot marked the position of the *unextinguished* ray, while no effect whatever was produced by the *extinguished* ray. A film of mica was now introduced into the course of the violet ray; two faint luminous images appeared, and two dark impressions were obtained. Those experiments were again repeated beyond the extreme violet ray. That part of the spectrum which traversed the prism of calcareous spar gave no luminous image, but the result was the same; to wit, the unextinguished chemical ray gave a dark impression on sensitive paper, and the extinguished ray none; and when a film of mica was used, two dark impressions of both rays were obtained.

The experiments with the rings were also tried; but although the impressions were visible, they were by no means so distinct as those obtained from the direct sun-light, a circumstance which is partly to be attributed to the great difficulty of keeping the axis of the apparatus employed in the axis of the polarized ray. The Sun's motion has to be compensated by the movement of the hand; and these experiments are, on this account, of difficult performance, unless a heliostat, or some similar contrivance, be used to keep the sun-beam precisely in the same direction during the required time.

## 2. Polarization of the Chemical Rays by Reflexion.

I have now stated the principal results at which I have arrived in the polarization of the chemical rays by double refraction, and shall next proceed to detail experiments which go to prove that these rays can be polarized by reflection. The apparatus made use of consists of a mirror composed of nine parallel plates of glass, by which a beam of polarized light can be thrown upon an analyzing plate of thick flint glass, so mounted that its angle of position can be changed, and its plane of reflection made to revolve round the polarized beam. The image of the Sun, after having been reflected from the mirror at the polarizing angle, was thrown upon the analyzing plate, the plane of which had previously been turned at right angles to the plane of primitive polarization. From this plate the ray was received upon a piece of photogenic paper, and in three minutes a very faint impression was obtained. The apparatus being still in the same position, another piece of paper, cut

from the same sheet, was substituted for that used in the last experiment, and the ray was depolarized by interposing a plate of mica between the mirrors; in three minutes the paper received a dark impression, thus affording another illustration of the effect of crystallized plates on the polarized chemical rays.

Another piece of paper, also cut from the same sheet, was made use of, but the analyzing plate was turned  $90^\circ$  upon its axis, before the ray reflected from it was allowed to fall on the paper, and in three minutes an impression was obtained, equal in intensity to that produced in the last experiment.

These three experiments, which occupied little more than ten minutes in their performance, were executed at a time when the sun's rays were of equal intensity, and the paper used was also of equal sensibility; and they afford an additional proof of the similarity of effect produced by polarizing forces on the chemical and luminous rays.

### 3. *Polarization of the Chemical Rays by repeated single Refraction.*

To exhibit this phenomenon I prepared two bundles of mica plates, nine in each bundle. These were arranged diagonally in a tube, one half of which could be turned round within the other. The tube was turned so that the planes of both bundles were at right angles, and the sun's rays were transmitted through it so as to fall on sensitive paper. In a few minutes little or no effect was produced, but on turning the planes round so as to coincide, an immediate darkening of the paper took place.

I next employed this method to polarize the chemical emanations proceeding from the sky alone; but it was necessary in this case to have simultaneous results, to observe the effects of varying intensity, two sets of experiments were carried on at the same time. A piece of thin window glass was chosen, out of which sixteen plates, one and a half inch long and one inch broad, were cut. These were arranged diagonally in four bundles, and placed in two tubes; two of the bundles having their planes coincident in one tube, and the other two with their planes at right angles in the other tube. The tubes were placed close to each other, in a perpendicular position in the open air, so that the light from the sky could pass directly through them upon two pieces of photogenic paper, cut from the same sheet, and placed so as to receive the chemical emanations. In this position they were left for two hours, in a tolerably clear day; and although the chemical

rays had to pass through the same number of plates in both instances, the impressions received by the paper differed much in intensity, that under the tube containing the two bundles, with the planes at right angles, being much less effected than the other. The explanation of this phenomenon is, that all those chemical emanations which were polarized by the repeated refraction of the first bundle of glass plates, did not pass through the second bundle when their planes were at right angles, and consequently produced no effect on the paper; but on the contrary, they passed readily through the second bundle, when its plane coincided with that of the first, and produced their characteristic darkening effect. The phenomena are in fact similar to those observed with the luminous rays under the same circumstances.

Such, then, are the results at which I have as yet arrived in this interesting branch of physical research, and they appear to prove that the third great division of the solar emanations, like the luminous and calorific, are capable of being acted upon by polarizing forces, and that thus they are all subject to the same beautiful laws.

Before concluding this communication I may state, that the photogenic paper employed in the experiments was prepared in the usual way with chloride of silver, but that it would be more satisfactory to use small Daguerreotype plates, particularly in obtaining impressions of organized or crystalline structures by the solar Microscope. To effect this, a pair of short polarizing prisms, made according to Mr. Nichol's improved plan, may be adapted to the Microscope, one being placed so as to polarize the Sun's light before it falls on the object, and the other to analyse the beam immediately after it has passed the object-glass. A sensitive surface placed so as to receive the image thus formed, would take a corresponding impression of the structure.

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*A New Process of Iodizing. For Plates intended to receive Photographic Images. Letter from M. Gauden.*

Having obtained photographic images as I stated in a preceding communication, with the aid of light alone, and without the intervention of the mercurial vapour, I was confirmed in the belief that the principal phenomena was the formation of an insoluble sub-iodide of silver. I then attempted an experiment in order to decide the question, and its complete success has sanctioned this theory.

I reasoned thus: if light takes away the iodine from the iodide of silver, a new exposure to the vapour of iodine should efface it, and the impression of the light besides that; I then exposed for several minutes, to the direct rays of the sun, a plate so prepared with iodine, that it had acquired on one of its parts a very strong tint, having covered the other part from its action with great care; I then exposed this plate, thus modified, to the action of chloride of idine; and finally I placed it in the dark chamber and submitted it to the mercury as ordinarily performed. I thus obtained a proof on which the most practised eye could not distinguish any difference between that part previously blackened by the rays of the sun, and the other half which had been deprived of this action; it is only just possible to see between them an infinitely small line of demarcation.

In the second place, I blackened by the direct solar rays as in the preceding experiment, a plate iodized to that degree which gives it a clear yellow colour; I then submitted it again to the action of the iodine, until the red stratum has formed. This plate, exposed in the chamber and to the mercurial vapour, as ordinarily done, gave me a proof in one minute, which without this operation would have taken 3 or 4 minutes; thus, far from enfeebling the sensibility of the iodide of silver, the preceding exposure to the light increased the sensibility, provided that in all cases at the close of the operation we avoid, as we are already acquainted with every access of the light. I have always presented to the rays of the sun during one second, a plate prepared with chloride of iodine; and after a new exposure to the chloride of iodine, I have obtained proofs which indicated at most a diminution of sensibility entirely without doubt, because I had not been able to destroy, by a new exposure to the chloride of iodine, the effect of the solar light without considerably augmenting the thickness of the stratum, and thereby alone diminishing its sensibility.

Thus it is evident that we may henceforward iodize the plates in open day, or even if we wish to put it to the most rigorous test, we may accomplish it even in the sun; provided that at the end of the operation, we operate in the dark.

Proceeding in this manner, I obtained yesterday, in two seconds a very fine proof from animated nature.

The attentive observation of the first layer of iodine, on white paper fully exposed to the light of day, is of the highest importance, inasmuch as it gives an opportunity of discovering on the plate the least defects of preparation, and

of judging well of the change of colour, effected at a later stage of the process by the chloride of iodine.

The plates prepared with chloride of iodine are susceptible of giving with the red glass, proofs formed in 1-15th of a second, nevertheless these proofs are almost always of a violet colour, whether it is that the red glass leaves in passing some exciting rays, or whether the plate in spite of all my cares, was previously impressed. With yellow glass the violet is still more decided, and frequently the plate submitted to the insulation becomes blackened in a few minutes, over its whole extent of its surface, whilst with old iodide of silver, at the end of two other hours in the sun, the black is still more intense. Having put in my apparatus a diaphragm presenting four times less surface than the portrait diaphragm, not permitting the light to penetrate it except during half a second, I have constantly obtained, with the red glass, very strong images, but presenting the aspect of proofs *toasted to the maximum*, and not being, on this account presentable ones.

The yellow glass acts in such a manner on the iodine by the process of Claudet, that I have constantly obtained, with the light obscured, passable proofs, always protecting my object with a yellow glass; a fact which has made me believe that the plates thus separated are sensible to the yellow rays, and susceptible, in consequence of giving proofs with artificial light, and especially with the sidral flame, which, notwithstanding its great apparent whiteness, gives the solar shadow of a pure yellow tint.

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*Letter to the Editor from W. H. Weekes, Esq. Surgeon, Lecturer on Philosophical and operative Chemistry, &c.*

SANDWICH, 9th. August, 1841.

My Dear Sir,

I am gratified to learn, by a notice prefixed to the last number of your ANNALS, that you are employed in collecting information respecting the numerous and interesting Electrical Storms of the present Summer. You have already done me the honour to put on record my communion relative to the great Electrical disturbance, witnessed here on the 9th. of May last, and if the subjoined additional notices of some subsequent atmospheric phenomena, should be found suited to your present views, I shall have much pleasure in placing them entirely at your disposal.

A Scientific friend, who dates from "Boulogne sur Mer Monday morning, July 12th. 1841, writes me thus:—

"My present object in writing, is, to ask whether your atmospheric Apparatus has given any and what signs, during the extraordinary changes we have had within the space of the last week? Sunday the 9th. there was a most violent Storm of Thunder at Paris, after a terribly sultry day; and about one o'clock in the night of Wednesday, two shocks of an earthquake were felt there. On Saturday evening last, July 10th. there was *here* a sudden fall of the barometer, of I think, *above half an inch*; and about three hours afterwards, a tremendous Whirlwind, with a Storm of Thunder and rain." A few brief extracts from my daily Register, will serve to show the degree of coincidence on this side the Channel, with the phenomena observed on the Continent at about the same periods.

*JULY, Sunday the 4th.* Thermometer 72°; Barometer 29, 65 at 2 p. m. Wind W. in mere Zephers; beautifully diversified modifications of Cirrus cloud; Cirro stratus and a few rain drops at eve; very slight divergence of Electroscopes—almost constant until Sun-set; current positive.

*Wednesday 7th.* Thermometer 64; Barometer 29, 33. Wind brisk from W. S. W. Cumulo floccossus in blue sky. Frequent free divergence positive.

In these two instances no remarkable analogy is apparent between the Electric condition of the atmosphere here and at Paris, though it will be perceived, that a considerable quantity of free Electricity was manifesting itself about this time, through the medium of the Sandwich apparatus; and I may remark, that the Thermometer on the 4th. reached several degrees higher than on any other day of the month, the average temperature of which did not exceed 65, with the Sun above the horizon.

On the 9th. we had an uninterrupted free positive current, with powerful discharges of sparks, during hasty showers and smart breezes from the N. W. At night Thunder, from clouds too remote for observation.

In reference to SATURDAY the 10th. and the following day, I subjoin an exact transcript of my memoranda.

10th. Thermometer 63. Barometer 29, 50; at 2 p. m. Breeze W. Blue sky with Cumulus and Cirrus clouds occasionally. Frequent free divergence positive; 9 hours 40 minutes, p. m. Brisk wind W. S. W. Haze upwards from thin sheet of Cirro stratus planus. Free divergence positive, with pulsatory and heaving motion of the electroscopic pendulums. It is unusual for the apparatus to shew any symptoms of elec-

tric action during a hazy state of the atmosphere. The *heaving*, and, as it were, troubled condition of the electroscopic pendulums is almost a certain indication of an approaching storm. Early in the following morning, four hours and forty minutes subsequent to the last mentioned entry in Register, the following memoranda occur. 11th. 2 h. 20 m. a. m.. Thermometer 49. Barometer 29.02. Wind gusty W.S.W. Hail shower and heavy rain.—Very distant Thunder. Powerful torrents of sparks from terminus of Apparatus; positive and negative alternately. Here we have a striking concurrence in regard of time, the falling of the Barometer, (nearly half an inch) and the “Storm of Thunder and rain”—as described by my Correspondent in France.

I shall submit to you a few more memoranda from the same source as the preceding, because they tend to show the highly-Electrical condition of the atmosphere during the month of July.

13th. 12 h. 32 m. p. m. Smart breeze W.S.W. A nimbus cloud hangs immediately over the line of atmospheric wire. Rain, hail, and clap of Thunder. Torrents of powerful sparks. Current negative in commencement, but frequently changing its character. An Electric wave closely following Thunder-clap, and occasioning the usual hissing noise *during 5 or 6 seconds*. Free disengagement of ozone.

14th. 6 h. p. m. Abundant rain; continuous currents of small sparks during 36 minutes, followed by moderate Electroscopic divergence (negative) at times.

15th. Free divergence (positive) as successive clouds approach zenith of Apparatus.

12th. 20 m. p. m. Increasing breeze. Viscinal Thunder, ragged nimbus clouds without rain. Electric waves repeatedly rush through apparatus with the most splendid effect. Powerful discharges of sparks followed these phenomena—character negative.

1h. 50 m. p. m. Gentle rain: discharge of sparks continues, altering from the negative to the positive condition.

3h. 55 m. p. m. Cessation of Electric current, after an uninterrupted duration of *three hours and twenty-five minutes, in the form of dense sparks, striking one inch and three-quarters*.

I shall not trespass further particulars on your attention, but merely state that subsequent to the 16th. scarcely twenty-four hours elapsed throughout the month, without free Electroscopic divergence, and frequent torrents of sparks from the hasty rains, and flocculent or mountaneous modifications of cumulous cloud.

I am, My dear Sir, very sincerely, Yours,  
W. Sturgeon, Esq.

W. H. WEEKS.

*Another letter from the Same to the Same.*

SANDWICH, 11th. August, 1841.

My Dear Sir,

In closing my communication to you on the Evening of the 9th. Inst. I could not anticipate the occasion for addressing you again thus early. At about 4 p.m. on that day we were visited by a hasty shower of heavy rain, which yielded me a torrent of powerful sparks, during somewhat more than half an hour; immediately after which every vestige of cloud disappeared, and a beautiful transparent blue sky succeeded throughout the following night. Invited by its delightful serenity, about the hour of eleven, I had walked out accompanied by a friend: the Moon had just entered her last quarter and was rising with a picturesque effect at the moment. My companion had addressed a question to me, which occasioned the direction of our eyes to the northern hemisphere., when we beheld at an altitude about mid-way between the horizon and the constellation of the Little Bear, the flight of a brilliant shower of small Meteors, passing from E.N.E. to W.S.W. or nearly so; and apparently leaving luminous trains parallel to each other and to the horizon for the space of a second or two. The latter circumstance might be merely a delusive effect produced by the sudden impression upon the retina. I have no precise idea as to the number of meteoric bodies thus unexpectedly presented to the organs of vision; it might amount to some twenty, and I am of opinion that it exceeded rather than fell short of that amount.

It immediately suggested itself to my mind, that the beautiful phenomena we had thus accidentally witnessed, constituted the prelude to the annual appearance of the "meteoric shower," now presumed to occur in August as well as November; a fact which I hope, owing to the very favourable condition of the atmosphere, must have been verified by other observers in the present instance.

Finding, in the space of a minute or two after the disappearance of the first flight, that other similarly luminous bodies, by one, two, and three identically, were starting out from the sky in the same direction, I requested my companion to turn his attention to the southward, while I kept my eye steadfastly towards the northern hemisphere. Within the space of thirteen minutes, seventeen other Meteors were distinctly counted by myself, passing uniformly in the direction of the shower



first seen, while an additional four were reported by my companion, which he said appeared to have proceeded from the eastward, and passing near his zenith, were lost in the south-west. As the Moon gained in altitude, the recurrence of the Meteors to the eye became proportionally lessened, only those of the greatest magnitude probably escaping the influence of her superior light; but I have no hesitation in stating that between the hours of eleven and one, considerably upwards of one hundred of these Meteors might have been counted, all proceeding, as it were, from a common centre, the position of which seemed uniformly to form a point in the heavens, some thirty degrees of a circle to the right hand. and below the constellation Ursa Minor.

I had last night hoped to renew my acquaintance with these beautiful phenomena, but experienced instead a densely clouded sky, with a Storm from the south-west, accompanied by drenching rains and powerful Electric currents.

I am, My dear Sir, very sincerely Yours,

W. Sturgeon, Esq.

W. H. WEEKES.

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MANCHESTER.—On Wednesday and Thursday, the 23rd and 24th of June, terrific storms of lightning, thunder, and heavy rains, visited this town and neighbourhood, of a more terrific nature than have been known for many years. On Wednesday the thunder rolled in heavy peals during the whole of the morning and forenoon; and about one o'clock in the afternoon, a most vivid flash of lightning accompanied by an almost simultaneous smart clap of thunder, alarmed the inhabitants of Greenheys, and all the southern boundaries of the town. Several persons felt a strong conviction that they saw the lightning strike the ground at the distance of a few fields from them, and felt certain that some damage must have been done in the neighbourhood; but as we have not heard of anything of the kind having happened at that short distance from the observers, it is possible we think, that the lightning struck some open field, all of which in the neighbourhood of Greenheys are excessively wet; indeed, incessantly soaked in water; and consequently well calculated to receive a discharge of lightning from the clouds.

On Thursday the 24th. the Electric Storm re-commenced; and if possible, with still greater severity than on the previous day. The Lightning struck a tall chimney in the Soap-works

of Mr, Jephtha Pacey, Strangeways, and produced some very remarkable effects. The chimney served as a general flue for several fire-places or ovens, over which are placed the boilers in which the materials for making Soap are boiled. The fires were in full play, and several workmen busily employed in different parts of the grounds. The first effect experienced by these men, was likened to a sudden strong blast of wind which blew them all *outwards*, as if from a centre, which was the chimney. Some of the men were suddenly removed by this expanding current, to the distance of ten or more yards from the place where they had been previously standing: and one man, who was working in a shed at the distance of nearly twenty yards from the chimney, was thrown against the wall of the building by this expanding force.

None of the men saw any Lightning, nor did they hear any Thunder at the time. The fact is, they were so completely astonished at the effect produced on themselves, that their senses very probably, suffered a momentary cessation, and unconscious of "passing events." It was fortunate, however, that some of them were thus removed from their former standing places, for the bricks came rattling down from the chimney to the very spot where they had been standing, and must necessarily have injured them severely had they remained there. Bricks were also thrown on every side into the yard to a great distance: on one side a large iron water tank was nearly filled with them. The iron doors of the ovens or fire-places were all blown open, and some of them thrown off their hinges. The chimney was rent on every side in various places, and the displaced bricks which had not fallen were leaning outwards. Some of the vertical rents were more than two yards long, and the bricks on both sides hanging by a mere point, the principal part of the mass being directed outwards. We visited this scene a few days after the occurrence, at which time the chimney was in the precise condition above stated. One of the workmen, a very sensible man, gave us all the other particulars. We learned also, that two boys who were sheltering from the rain at some distance, saw a fire-ball, as they called it, descend obliquely to the top of this chimney, and that the moment it arrived there, it spun round from the top of it, like a cork-screw, down to the ground.

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*Rochdale.*—During the severe thunder storm which visited Rochdale and its neighbourhood on saturday afternoon, June 26th. two cows, one valued at £13, the other at £15, were

killed in a pasture field at Butterworth. The earth near to the place where they had been grazing was torn and ploughed up in a very remarkable manner. The cows were upwards of four yards asunder. The lightning was very vivid in the town of Rochdale where it did considerable damage. The electric fluid struck, and passed through the roof of a cottage at Smallbridge, inhabited by a woollen weaver, named Abraham Mills, and splintered into small pieces a spinning jenny which was in the upper story, strewing the fragments about the room. The wife of Mills was weaving in the room beneath, and the shock flung her on the floor; it also knocked down a woman who was at work in the adjoining house. At Dobfield, in Butterworth, the lightning was seen to descend above an oak tree which stood in an open field, on coming near the tree it took a side direction, spun round it like a corkscrew, tearing off the branches, and strewing them on all sides to a great distance.\*

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*Awful Storm in Birkshire.*

*Wednesday, May 29th.*—Last night the Forest Division of Birkshire was visited by one of the most terrific thunder and hail storms which this country has ever witnessed. The sky, which for the last two days had been unusually bright and sultry, began about eight o'clock in the evening to present symptoms of a change, and to forebode the coming tempest. As the night closed in, the atmosphere became more close and oppressive, and masses of dark and threatening clouds began to collect from the south-east, illuminated almost incessantly with vivid lightning, accompanied by the most terrific crashes of thunder. Up to this time there had been no fall of any kind; but, about half-past nine, without a minute's warning, the most destructive shower of hail was discharged that it has ever been our lot to record. We have been favoured with an account of the storm, by one family who were spectators of the scene. They were sitting at the tea-table, and the servant, who had fastened the shutter in two of the rooms, was preparing to close the others, when a crash was heard, as if the side of the house had fallen in: and in an instant, the shutters were burst open and the room covered with pieces of ice, many of which were found under the

\* We have here another instance of the effects produced on inferior conductors by oblique discharges of Lightning. The branches of the tree were scarcely injured, only at the point where they were severed from the trunk and the spiral descent of the lightning arose from the composition of two forces, as in the case of the chimney.—EDIT.

sofa half an hour afterwards, as large and nearly as hard as marbles. To close the shutters a second time was found to be impossible, as the lightning almost blinded those who attempted it, and the shower of hail was irresistible. In the bedrooms where the windows had been left open, the chairs thrown down, the beds drenched with hailstones, and the floors covered with ice and glass, After raging in this awful manner for about twenty minutes, the storm abated.

The chief weight of the tempest seems to have fallen on the country between Bracknell and Reading, a distance of about eleven miles. In Bracknell and Workingham, the windows on one side of the street in each been literally smashed in, and are likely to afford work to the glaziers for three months. At Easthamstead Park, the seat of the Marquis of Downshire, great mischief has been done to the mansion and the conservatories. At Bill Hill, the seat of Mr. Leveson Gower, the damage is immense, nearly all the windows on one side of the house being broken. At Bear Wood 160 panes are broken on one side of the house, besides all the glass in the conservatories and skylights. At Hurst House, the residence of Sir John Conroy, the damage is said to amount to nearly £1,000. At Sindlesham House, the residence of Mr. T. Harman, 136 panes are broken in the house, and the glass in the conservatories quite destroyed. In the Berkshire Hospital at Reading, 150 panes are broken; and at Purley, near Reading, we hear that in addition to other damage several cows have been destroyed.

The ravages of the storm out of doors are not less complete, and are far more distressing than within doors. The devastation of the crops is immense; whole fields of beans, peas, and turnips, besides wheat, are as completely levelled as if they had been cut off with a scythe, and several small farmers will probably be ruined. The flower gardens present a most melancholy spectacle, being completely strewn with the wrecks of their luxuriance, and the shoots of the young forest trees are in many places quite cut off.

The visitation has certainly been most awful, and will be long and painfully remembered. We are thankful to add, that we have not heard of the loss of any lives, nor of the occurrence of any personal injury. *Correspondent of the Times.*

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**THUNDER STORM—SHEFFIELD.**—On Thursday last July 1st. the electric fluid descended and entered the Royal Oak public-house in Pond-lane, where it fused the barrel of a gun

in the garret, and burned a quantity of feathers in a bag; it also melted or dislodged the bell wires, and some other similar fixtures, but providentially did no injury to any person in or about the house. At the same time, the lightning struck and killed a horse belonging to Mr. Slater, butcher, in a field near the Cricket-lane, in the Park. About eight o'clock on Friday evening, a stack of chimneys, in Gibraltar-street, between two public-houses, occupied by Mr. Kay and Mr. Ralph Hall, were also shattered by the same subtle element, which entered into the rooms and caused considerable devastation, especially in Mr. Kay's house. Happily no personal damage was sustained by the inmates.—On Wednesday evening, 23rd ultimo, the village of Edinstowe and its forest was visited by a tremendous storm of thunder, lightning, and hail, accompanied with heavy rain; the thunder was most terrific, peal succeeding peal with awful crash! A large oak tree at the village of Budby was struck with the electric fluid, and literally rent to pieces; the top was struck off about seven feet from the ground, and scattered in fragments in a circle extending at least fifty yards from the trunk; one large bough weighing four cwt. was thrown a distance of seventy yards. The part of the trunk left standing had every particle of bark removed, as if it had been peeled off with the greatest care. Fortunately no serious damage was done.

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**SINGULAR EFFECT OF LIGHTNING ON GLASS.**—About noon on Thursday, July 15, this town was visited by the storm which we find was so general throughout the country. The peals of thunder were awfully loud, and in several instances succeeded the lightning without a moment's interval, showing the close proximity of the discharge. Several haystacks and barns were fired in different places, as mentioned in the letters of our correspondents. The County Lunatic Asylum, on Barming Heath, was struck by the electric fluid; but no material damage was sustained. A chimney-pot was shattered into fragments, and a stack of chimneys were slightly injured, several of the stones being partially displaced. In Maidstone, the lightning struck a chimney about 14 feet high on the pipe manufactory of Mr. Britter, near the county gaol, and produced some singular effects. A brick at the corner of the chimney was removed about an inch from its place, and the edge of the brick adjoining it was chipped off as if from the blow of a hammer. The fluid then fell on

the tiles just below, three of which were removed; and it appears as if it had been dispersed, and had been projected against the dwelling house opposite, the four windows of which, facing the chimney, and at about 15 or 20 feet distant, present a very singular appearance. In several panes oval blisters have been raised like air bubbles in badly manufactured sheet glass. When first observed, they were covered with a fine dark coloured powder, which was unfortunately brushed away without being minutely examined, and there is a similar appearance inside some of the blisters. When the building was struck, four men at work in the manufactory were knocked down, and were of course dreadfully alarmed. Mr. and Mrs. Britter were sitting in a room facing the workshop, and they describe the lightning as darting by them like a ball of fire. Happily they were uninjured. The storm lasted nearly three hours, it having returned for a short time after its first visit prior to its entire cessation.—*Maidstone Paper.*

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**THUNDER STORM AT LIMERICK** About five o'clock in the afternoon of Friday, June 25th. a Thunder-Storm visited this City, the Electric power of which surpassed any thing of the kind in memory, and the darkness was awful. The rain rushed down with impetuous force, mingled with hail-stones, and the Thunder told its near approach, with a noise and effect equal to that of a cannon, as if absolutely tearing up the very pavement of the streets. The sensation was profoundly grand and sublime. A portion of the Electric fluid struck the angle of one of the main walls of Ballymacreese-house, the residence of J. Shine, Esq., who was then sitting up stairs in the parlour with two ladies and some of his children. The Lightning shattered a wall three feet thick, ran down the chimney, and burst into the parlour with an astounding shock, displacing a large stone over the mantle-piece, but then most providentially was attracted at this moment by the bell-wire, along which it shot down through the floor into the lower rooms, following close this miraculous conductor, burning the paper on the walls for several inches on both sides, and melting the bell-wire, leaving a most sulphurous smell throughout its rapid progress, accompanied by a dense mist or smoke. The servants in the kitchen were knocked against each other and fell, and a girl in a distant room of the house had some glass broken in her hands, in the act of removing to the pantry. The stunning shock of this dreadful visitation was felt for hours after by Mr. Shine and

all the inmates, who have to thank Heaven for their escape under circumstances so awful and extraordinary.—*Limerick Chronicle*.

On Tuesday Morning, August 24th. the lightning struck the steeples of two Churches in Liverpool.

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THEORY OF WATER SPOUTS AND TORNADOES.

( *From silliman's American Journal of Science.* )

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A few weeks since a large kettle of water, which having been used for washing, was covered with a thick smooth scum of curdled soap, was hanging over the kitchen fire, and though there was no ebullition, a dense volume of steam was rising from it, and with a rapid whirling motion ascending the chimney. My attention was drawn to it from the fact, that the movement of the steam was affording additional proof of the general course of all atmospheric currents from right to left, according to the theory of Mr. Redfield, a theory, of which thus far, we have frequently noticed the verification. The steam whirl formed immediately over the surface of the kettle, and made a column some two or three inches in diameter, for about eighteen inches in length, when it disappeared behind the mantle of the chimney. In the centre of the whirling column of steam, which rotated with astonishing rapidity, a clear space could be seen, distinctly marked by a difference in color, showing that the pillar was a tube. As in obedience to the different currents of air in the room, the column changed its position over the surface of the kettle, we observed the movement was accompanied at times with a curious agitation, which at first was supposed to be mere ebullition; but from its being always under the centre of the column, being most violent where the whirl approached nearest its surface, and shifting position with it, was soon perceived to be owing to that.

The appearance was as if a man's hand was moved under the surface, at times protruding his fore finger upwards, and lifting the scum, or rather forcing the finger through it to its full extent. It occurred to us at once as a fine illustration of the commencement of a Water-spout, and we continued our examination for some time until the general motion of the surface by boiling prevented any very marked action of the whirl. As the rising fluid evidently ascended the clear space in the centre of the cone or column, it was certain that the column was hollow, and that within the whirl there was a diminished atmospheric pres-

sure. During the times that the water mounted the highest, (which was between four and five inches) there was a violent agitation of the surface in the immediate vicinity of, or beneath the base of the rotating column, and a careful examination showed that small pieces of the foam were occasionally wrested from the upper part of the rising water, and instantly disappeared. It could not be seen that there was any distinct rotation given to the elevated water, which swayed and bent with column of steam.

It appeared to us that from this incident, simple and trifling as it may appear in itself, some valuable inferences may be drawn. The origin of water-spouts, in connection with Whirlwinds, and the laws that regulate the ascent of water, were well exhibited. That water should ascend to the height it evidently does in Water-spouts at Sea, by atmospheric pressure alone, is not to be supposed; but it is atmospheric pressure that forces the water into the hollow at the base of the cone, and places it in a position to be first acted upon, and then lifted by the whirling air. When once the upward current is established, there seems to be no difficulty in continuing it; and as the water thus lifted must return to the earth, by being thrown without the upper circumference of the whirl, or when the column is suddenly separated, by pouring downward with the same volume with which it was rising, it accounts for the deluges of water that at times accompany Water-spouts.

The action or ascent of the water within the tube also showed that the atmospheric pressure was greatly lessened or removed in the interior of the whirl, and thus explained satisfactorily many of the phenomena that accompany Tornadoes or Whirlwinds. Thus it has always been found in violent Tornadoes, that the windows or gables of buildings that were near the centre of the line of the Whirl-wind, are almost invariably burst outwards, and frequently directed in the face of the advancing Storm. This was particularly noticed at the destruction of Natchez, and at Shelbyville, and cannot be well accounted for in any other way than by the violent expansions of the air within the buildings, when the outer pressure is suddenly taken off.

In many Storms or Tornadoes, the Thunder does not appear in distinct explosions, nor the Lightning in separate flashes. On the contrary, there is a continued blaze of fire in the cloud, and the roll of Thunder is incessant. In such cases, effects are observed which indicate in the line of the Storm, the continued action of electric energy, and give reason to suppose that the ascending column produced by the Whirl, forms a perfect conductor, along which the electric fluid descends



continuously, and not in successive masses. Thus in most Tornadoes, the trees within their range that are not torn up have their leaves scorched and withered as if a fire had passed over them; and iron substances, such as farming implements, always exhibit unequical evidence of having been submitted to Electrical action. This was particularly noticed in the Tornado near New Haven. That such is the case, the fact, that in such Tornadoes occurring in the night, the central part of the Whirl appears like a pillar of fire or heated Iron, is conclusive evidence. Of such appearance the Tornado at Shelbyville, and the one described by Arago near Paris, are examples. If a stream of smoke from a chimney, or a column of heated air from grain or hay in a Barn, are such conductors, as experience shows them to be, there can be no doubt that such a column as is formed in a Whirl-wind, reaching from the Earth to the Heavens, would form one still more efficient.

OTISCO, N.Y. Jan. 1841.

W. G.

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## PROCEEDINGS OF THE ROYAL SOCIETY.

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### SELECTIONS.

February 4.— A Paper was then read, entitled “On Some Electro-Nitrogurets.” By Wm. Robert Grove. Esq., M. A. F.R.S.

The author states that he has made attempts to render permanent the ammoniacal amalgam, and that he has succeeded in freezing it by means of solid carbonic acid, during which solidification, and also while in its solid state, it underwent no chemical change. He subsequently attempted to procure a permanent compound by electrolyzing a solution of hydrochlorate of Ammonia with an extremely fusible alloy at the cathode; but this attempt was unsuccessful. It then occurred to him, that by using an oxidable metal at the anode, which could be revived in conjunction with nascent hydrogen and nitrogen at the cathode, one or both of these elements might be combined with the solid metal, and so form permanent compounds.

The experiment made in this manner with the metals zinc, cadmium, and copper, was perfectly successful. A spongy mass collected at the cathode, which floated upon the liquid, and which, when washed and dried, was analysed by heating

in a tube retort; five grains of the zinc compound gave 0.73 of a cubic inch of permanent gas, which on examination proved to be nitrogen with one-fourth hydrogen. The same quantity of the cadmium compound gave 0.207 cubic inch of nitrogen, with no admixture of hydrogen. A like weight of the copper compound gave 0.107 of nitrogen. No ammonia was evolved from either; and the author is inclined to think, that the hydrogen yielded by the zinc compound, resulted from the reaction of the metal upon combined water. The specific gravity of specimens of these substances, which the author tried were respectively 4.6, 4.8, and 5.9. A mixed solution of chloride of gold and hydrochlorate of ammonia, electrolyzed with platinum electrodes, gave a black powder of the specific gravity 10.3; five grains of which, being heated, gave only 0.05 cubic inch of gas. The author proceeds to observe, that the similarity in appearance and mode of formation of these compounds and of the mercurio-ammoniacal amalgam, is strong evidence of identity of constitution, and that the non-permanence of the latter substance is due to the mobility of the mercury for if we place the compounds in similar circumstances, that is solidify the mercurial one, or liquify those of the other metals, the phenomena are perfectly analogous. The experiments also bear immediately upon those of Thénard, Savart, and others, where ammonia, passed over heated metals, was found to be de-composed more completely by the oxidable than by the in-oxidable metals, and to alter their physical characters, without materially increasing their weight. On examining papers connected with this subject, the author found that Mr. Daniell had cursorily noticed a deposit somewhat analogous to those here treated of, which was formed upon the negative plate of his constant battery when this was charged on the zinc side with hydrochlorate of ammonia, and the nature of which that gentlemen observed was worthy of further examination, but had not time to investigate.

February 11.—A paper was read, entitled "Contributions to Terrestrial Magnetism," No. 2. By Major Edward Sabine, R.A., V.P.R.S.

This paper is the second of a series, in which the author purposes to communicate to the Royal Society, the results of Magnetic observations in different parts of the Globe, having for their object to supply the requisite data, for deducing the numerical elements corresponding to the present epoch of the general Theory of Terrestrial Magnetism. It consists of two sections: the first comprises the observations of Captain Belcher, R.N., and the officers of H.M.S. Sulphur, at twenty-

nine stations on the west coast of America, and the adjacent Islands, between the latitudes of  $60^{\circ} 21' N.$  and  $18^{\circ} 05' S.$  The second contains a new determination, by the same officers, of the magnetic elements at Otaheite, made in consequence of the discrepancies in the results obtained by previous observers, and of a note to M. Gauss's *Allgemeine Theorie*, in which Otaheite is spoken of as a highly-important station for the future improvement of the calculators of the theory. Abstracts are given of the original observations, which are deposited in the Hydrographic Office of the Admiralty, as well as a full detail of the processes of reduction by which their results have been computed. The values of the horizontal and total intensities are expressed in terms by which the results of observation are immediately comparable with the maps of MM. Gauss and Weber, in the "*Atlas des Erdmagnetismus*."

By an investigation into the "probable error" of a single independent determination of the magnetic intensity with Hansteen's apparatus, derived from the data furnished by Captain Belcher's observations, the author shows the extreme improbability that the differences in the results obtained at Otaheite by Messrs. Erman, FitzRoy and Belcher, should be occasioned by instrumental or observational error. They are also far greater than can, with any degree of probability, be ascribed to periodical or accidental variations in the magnetic force, from its mean value. The only known cause adequate to their explanation is, what may with propriety be termed *Station Error*; that is, local disturbing influences, in an Island composed chiefly of volcanic rocks, and where the spot of observation selected by the different observers may not have been precisely the same.

By a reference to the magnetic survey of the British Islands, the occurrence of station error is shown to be frequent in countries of far less decided igneous character than Otaheite; and that its existence may always be apprehended where rocks of that nature approach to, or rise through, the superficial soil. The absolute determinations of fixed observatories, are as liable to station error as those of the magnetic traveller, since no continuance or repetition of the observations can lead to an elimination of the error; it consequently presents a practical difficulty to the proposed determination of the elements of the theory from exact observation, at only a few select positions on the Globe. The remedy is to be found in the combination of fixed observatories and magnetic surveys; the observations of the survey, being made in concert with, and based on those of the fixed observatory, will be furnished thereby

with corrections for the secular, periodical, and accidental variations of the elements, and will consequently determine *mean* values: and a proper combination of the mean values thus determined, over a space sufficiently extensive to neutralize *district* anomalies, as well as those of a more strictly local character will furnish, in their turn, a correction for the station error, if any, of the fixed observatory.

## ELEMENTARY LECTURES ON ELECTRICITY.

### LECTURE XI.

From the successful experimental results which Franklin obtained in favour of his theory of the leyden jar; it was an easy step in the analogy, to suppose that two jars might be as easily charged as one jar only, by collecting the fluid which departed from the *outside* of that placed in connection with the prime conductor, in contact with the inside of the second jar, whose outer surface was connected with the ground: and the idea led to the establishment of new experiments of an exceedingly interesting nature. If, for instance, I insulate the jar a, fig. 1. plate VI. by hanging it on the prime conductor, as in a former experiment, and present the jar b, to its outer surface; the moment that the machine is put into motion a series of sparks are seen passing between the coating of the jar a, and the knob of the jar b, and when these sparks have nearly disappeared both jars are found to be charged; as we find by a proper application of the discharging-rod, first to the jar a, and as soon as possible afterwards, to the jar b.

This experiment is a beautiful exemplification of the Franklinian doctrine of the leyden jar as far as regards the departure of the electric fluid from the outer surface of the first jar a; and it may be shown by a further extension of the number of jars in sequence, that the same explanation applies to the most remote jar in the series.

We will now arrange a series of jars, a, b, c, d, &c. as in fig. 2, and we shall find that by such an arrangement the whole series will become charged at one and the same time, by the fluid which departs from the outside of the foremost jar a, being transmitted to the inner surface of the second jar b, and the fluid of the outside of b, being transmitted to the inside of c, and so on throughout the whole series, the last jar of which has its outer surface connected with the table, and consequently, *indirectly* with the ground, by means of the metallic chain or wire.

Now in order to be perfectly satisfied that every jar in the series is charged, I first separate them from one another and the jar a, from the prime conductor. I then apply the discharging rod to each jar separately, and we find that the usual phenomena accompanying a discharging jar is exhibited by every individual jar: and since the jar a, was the only one which could possibly derive its charge *directly* from the prime-conductor, the other ones must of necessity, have become charged by the fluid which departed from the outer surface of their immediate predecessors in the series.

There is still another method of illustrating this beautiful theoretical point by experiment. I will again arrange the jars in the same order as before fig. 2. and again put the machine into motion: and, if course, the whole series of jars become charged as decidedly as in the preceding experiment. Now, instead of separating them when charged, I will apply one ball of the discharging-rod to the chain hanging from the last jar d, and the other ball of it to the ball of the first jar a, or to the prime conductor in connexion with it; and by this means I discharge the whole series of jars at one and the same moment. By this arrangement I cause the jars to neutralize one another: for the redund-

and fluid on the inner surface of the jar *a*, rushes through the discharging-rod to the negative outer surface of the jar *d*: and the redundant fluid of the inner surface of *d*, supplies the deficiency on the outer surface of the jar *e*, and so on throughout the series: and although perhaps we may not find an exact equilibrium established on the two surfaces of every individual jar in the series, the process accomplishes their discharge as completely as by applying the discharging rod to each jar separately: and which is an additional proof, though an indirect one, of the jars having become charged by each others electric fluid: and it is a *direct* proof, as far as experiment is capable of proving any thing, that the restoration of a general equilibrium in the series, has been accomplished by a return of the previously expelled fluid, to its original abode on the outer surfaces of the jars.

If, instead of the jars touching one another, as in our last experiment, we were to place them at a short distance from one another, we should see a series of sparks traversing the thin plate of air interposed between the coatings of one of one jar and the ball of the other, during the whole period occupied in the process of charging; and when the discharge takes place by connecting one ball of the discharging rod with the chain attached to the coating of the last jar (*d*), in the series, and with the other ball approaching the knob of the first jar (*a*); the whole of the spaces between the several jars will become highly and simultaneously illuminated. This experiment has a very beautiful effect when performed in a darkened room. The noise produced will be in proportion to the size and number of jars employed in the series.

In all these operations of the leyden jar whilst in the act of charging, either *directly* from the prime conductor, or *indirectly* from the electrical influence of that apparatus, through the medium of other intervening jars; we find no difficulty in tracing a progressively increasing polarization of the opposite sides of each individual jar; which become positively and negatively electrical alternately, whatever may be the extent of the series: and when the polarization is at a maximum, the jar is as fully charged as the machine is capable of carrying on the charging process. Hence you will easily understand that the charging of a jar to a high degree requires a proportional facility of becoming polarized by the energies of the machine; which facility appears to be effected by an attention to two principal circumstances:—the thinness of the glass, and the connection of the outer surface with the ground; which circumstances are analogous to those that are required for the electro polarization of metallic bodies, as already illustrated in former lectures: for in those cases a thin plate of air between the bodies becomes electro-polar as decidedly as the metals themselves; and the facility of polarization is enhanced by the last metal in the series being in good conducting connection with the ground,

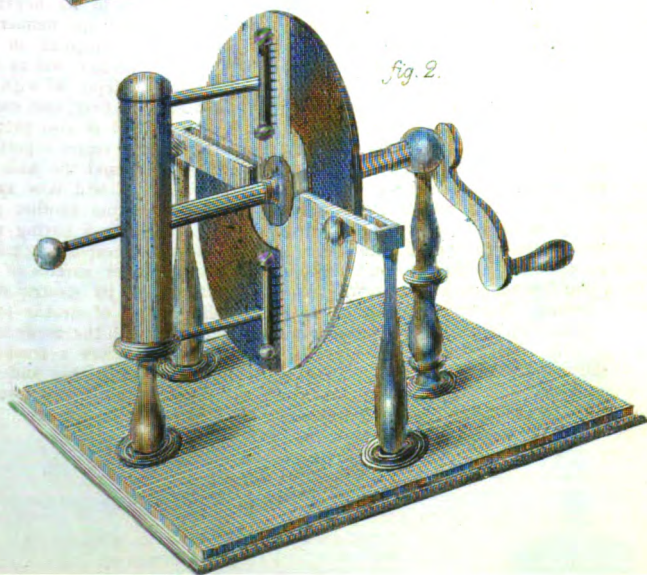
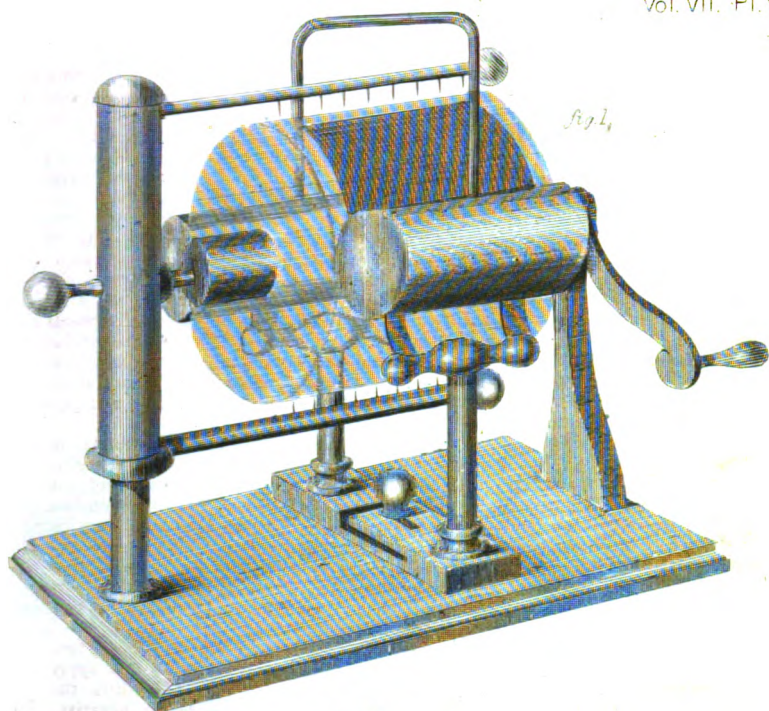
Another very interesting experiment is to be performed by having two jars so united with one another and with the prime conductor as to enable us to charge the one independently of the other. If, for instance we have a small metallic stage fixed to the wire belonging the inside of the large jar (*a*), fig. 3 plate VI. and upon that stage we place the smaller jar (*b*); we can very conveniently charge the large jar (*a*) by connecting its inner surface with the prime conductor, by means of a copper wire; and as the inner surface of the jar (*b*) is insulated it will receive no charge, with the exception of a small portion of the fluid which rests on its outer coating by being in metallic connection with the prime conductor and inner surface of the jar (*a*).

In this condition of things, the small jar, though not charged according to the general acceptation of the term, is nevertheless in a state of polarization; and its inner surface is negatively electric; for the fluid naturally belonging to that surface has been expelled as far as circumstances will allow, and a portion of it will be found in the ball (*c*), which is in an electro positive state. Hence, the metallic appendages belonging to the inner surface of the jar (*b*), are electro-polar and as the two surfaces of the glass are also electro-polar in the same direction, being under the same powerful polarizing influence of the accumulated fluid on the inner surface of the lower jar (*a*); the inner surface of (*b*) has a strong tendency to part with its own fluid, and is in an excellent condition to discharge it

suddenly to any negatively electric body that may be brought within the sphere of its influence. To show you this fact I place the lower ball of the discharging-rod on the metallic plate on which the jar (a) is standing, and then make the upper ball approach the knob (o,) of the jar (b); and again we observe a bright flash of light and hear a report,—the usual attendants of an electric discharge; which in this case has been a discharge of a portion of fluid from the inner surface of the jar (b.) to the other surface of (a.) Therefore both jars are now charged, the inner surface of (a.) and the outer surface of (b), being electro-positive, and their opposite sides consequently electro-negative: and both jars may be discharged either separately, or at the same moment; accordingly as we apply the discharging apparatus. If I join the inside of the upper jar (b), with the outside of the lower jar (a), by means of a metallic chain, or wire: and after placing one ball of the discharging-rod on this chain I bring the other ball towards the stage on which the small jar stands, or towards any of the metallic appendages joining the inside of the jar (a) and the outside of the jar (b): you find that the usual phenomena of discharge takes place: and by examining the jars afterwards we shall find that both are as neutral as jars usually are after the first application of a discharging rod.

Let us now charge these two jars again, by the same means as before, in order that I may give you a specimen of the curiosities displayed by discharging them separately. I will first apply the discharging rod to the outside of the jar (a) and then I bring the other ball of it to the stage connected with its inside, the flash and report are produced, and the jar becomes discharged by a metallic connection between its inner and outer surfaces. But the moment I remove the discharging-rod, the jar (a,) again becomes electro-polar by the electric influence of the upper jar (b). I now apply the discharging-rod to this jar in order to discharge it also: and by a few moments metallic connection of its two sides, we find that both jars are rendered nearly neutral.

I will now again charge both jars as before; but in the *discharge* I will proceed in a very different manner to any I have hitherto shown you. I first apply the discharging-rod to the two sides of the upper jar (b), and discharge it in the usual way, by transporting the redundant fluid from its outside to its negative, inside, and thus restoring the equilibrium of the two sides. But the moment I take away the discharging-rod the jar (b) again becomes electro-polar, in the same manner as when first under the influence of the lower jar (a): but as this latter jar has lost a portion of its contents by supplying the small jar (b) with the previous charge, the polarizing influence is not now so great as at first: and, consequently, the tendency of (b) to dispose of its inner-surface fluid is also proportionally less. However, you will observe a bright flash and smart report by again applying the discharging-rod to the outer surface of (b) and the ball (o) of the small jar (b); and the latter again becomes charged. I will now again discharge the jar (b) and render it neutral as before, by conveying another portion of fluid from its outer surface. The charge of the large jar (a) having now lost another portion of its fluid, has consequently suffered a corresponding relaxation of its polarizing influence, and communicates to the inner surface of the jar (b) a still less tendency than before, to dispose of any of its electric fluid to other bodies. Notwithstanding, however, this diminution of electric force in the large jar (a) by every successive partial discharge through the medium of the jar (b), the phenomena may be repeated many times over before a complete neutralization takes place; and [especially when the lower jar is large and the upper one very small. It is an experiment of great interest and highly favourable to the theoretical views which Franklin entertained of electric actions.

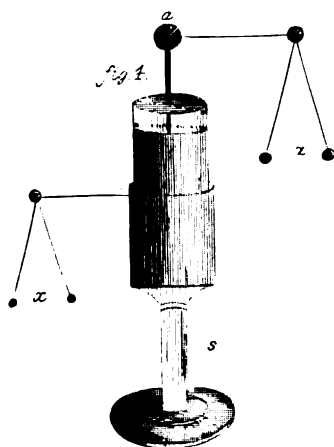
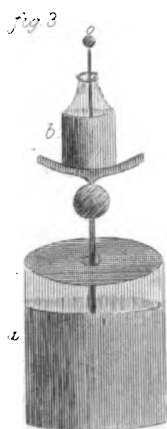
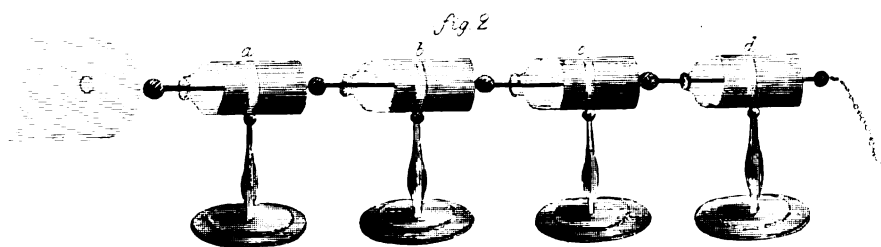
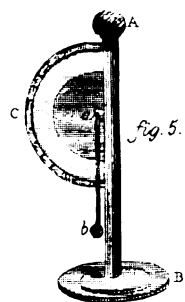
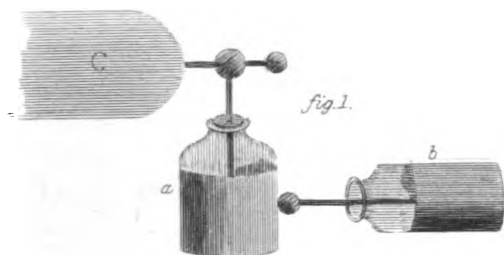


MR J. GOODMAN'S ELECTRICAL MACHINES.

*Engraved by J. H. St.*









THE ANNALS  
OF  
ELECTRICITY, MAGNETISM,  
AND  
CHEMISTRY:  
AND  
**Guardian of Experimental Science.**

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OCTOBER, 1841.

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XIV. *Researches on Heat—Fourth Series. On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat.\** By JAMES D. FORBES, Esq. F.R.S.S.L. & E. Professor of Natural Philosophy in the University of Edinburgh.†

*Arts. 1—12. Laminated and Smoked Surfaces. 13—29. Rough Surfaces. 30—34. Metallic and other Gratings. 35—53. Powdered Surfaces. 54—65. Conclusions.*

From the Transactions of the Royal Society of Edinburgh, Vol. xv. Part I.

1. On the 2nd. September 1839, M. Arago communicated to the Academy of Sciences of Paris, a Letter by M. Melloni, containing some very interesting experiments on the Transmission of Radiant Heat. M. Melloni finds that rock-salt (which is well known to transmit rays of heat from all sources yet tried with equal facility) acquires, by being smoked, the

\* The substance of the present paper was communicated to the Royal Society of Edinburgh on the 16th. December 1839, in the words of the memorandum which forms part of this Note. The memorandum itself was read, with some verbal explanation and citation of additional facts, on the 6th. of January. Every experiment to which reference is made in the present paper, was performed between the 12th. November 1839 and the 4th. March 1840. Since that time, I have not made a single experiment on the subject. Occupation of other kinds has prevented me from digesting, until now, the results of these experiments, and from stating the grounds of the conclusions which I formerly announced. The present paper, as it stands, having been submitted to the Council on the 15th May 1840, is printed by their authority. The following is the memorandum just referred to, reprinted from the Proceedings of the Royal Society of Edinburgh:—

“On the Effect of the Mechanical Texture of Screens on the immediate transmission of Radiant Heat. By Professor Forbes.—On the 2nd of September, 1839, M. Arago, communicated to the Academy of Sciences a letter by M. Melloni, containing some very interesting experiments on the transmission of Radiant Heat. M. Melloni finds, that rock-salt (which is well known to transmit rays from every source with equal facility) acquires, by being *smoked*, the power

power of transmitting, most easily, heat of low temperature, or that kind of heat which is stopped in greatest proportion in glass, alum, and (according to M. Melloni) every other substance.

2. In the Third Series of these Researches, § 3, I have at- of transmitting most easily heat of low temperature, or that kind of heat stopped in greatest proportion by glass, alum, and (according to M. Melloni) every other substance. The experiments contained in the Third Series of my Researches on Heat, show that this is equivalent to saying, that substances in general allow only the more refrangible rays to pass; and as M. Melloni had been led by his previous experiments to the same conclusion, his statement amounts to this, that, whilst rock-salt presents the analogy of white glass, by transmitting all rays in equal proportions, every substance hitherto examined acts on the calorific rays as violet or blue glass does on light, absorbing the rays of least refrangibility, and transmitting only the others.

"M. Melloni believes, that the first exception to this rule, or the first analogue of red glass, is rock-salt previously smoked. I desire, however, first to call attention to the fact, that, in a paper published in May 1838, (Researches on Heat, Third Series), I described a substance having similar properties, namely, mica split by heat to extreme thinness, such as I employ in polarizing heat. In the Month of March 1838, I had established by reiterated experiments, that the transmission of heat through glass, far from rendering it less easily absorbed by mica in this peculiar state, had a contrary effect, and also that heat of low temperature, wholly unaccompanied by light, was transmitted almost as freely as that from a lamp previously passed through glass.

"It even appears, from experiments I have since made with the same form of mica, that some specimens transmit *scarcely half* as much luminous heat previously passed through glass, as that from a body below visible incandescence.

"Mica itself, not laminated by the action of fire, possesses, as I have shown by contrasted tables in the paper referred to (Art. 23, 24), properties exactly the reverse; hence the effect is due to the peculiar mechanical condition of the body, and not to its elementary composition.

"It, therefore, at once occurred to me, on reading M. Melloni's communication, that the effect of smoking the salt might be merely owing to a mechanical change in the surface affecting the transmission.

"Roughening the surface was the most obvious experiment, and I found, as I anticipated, that heat of low temperature is very much easier transmitted by salt scratched by sand-paper in two directions at right angles, than luminous heat. Thus, a plate of salt which, when well polished, transmits 92 per cent. of heat derived from a lamp, and sifted by a glass plate, and also 92 per cent. of heat wholly unaccompanied by light, transmitted, when roughened, only 17 per cent. of the former and 45 per cent. of the latter.

"A thin plate of mica, when similarly scratched with emery-paper, so as merely to depolish it, transmitted much more nearly the same per-centage of heat from different sources than when *bright*; showing that the loss of polish affects the transmission of the more refrangible rays much more sensibly than that of the others.

"Yet this effect is not attributable to a variation in the ratio of the reflexion of heat of different kinds at the surfaces of the plate. For, in the *first* place, I have proved, and already communicated the fact to the Royal Society (see Proceedings for April 1839), that reflexion takes place at a polished surface, with almost, if not exactly, the same intensity for all kinds of heat; and, *secondly*, I have found, by direct experiment, that, at least for the higher angles of incidence, reflexion is most copious from rough surfaces for heat of low temperature, or the same kind which is most freely transmitted, proving incontestably that the *trifling* action of rough surfaces is the true cause of the inequality.

"That there is a real modification of the heat in passing through a roughened surface, as well as through laminated mica and the smoky film, appears from direct experiments which I have made on the heat sifted by these different media ;

tempted to demonstrate, directly and numerically, that the rays of heat which have passed through alum, glass, and indeed every substance which I tried, have a *mean refrangibility* superior to that of the rays before such transmission; and as M. Melloni had been led in a general way by his previous experiments to a similar conclusion, he inferred, and justly, that most diathermanous bodies absorb the less refrangible rays in excess, and therefore are to heat what green, blue, or violet diaphanous media are to light. Rock-salt alone (so far as we know) possesses the property of *indifferent diathermancy*, and is the single analogue of white transparent glass.

3 The generalization of this principle is a matter of much importance, and especially as it carries our knowledge a step higher in the scale of truth, by teaching us to refer to the quality of *refrangibility* certain properties of heat, which before were connected only with certain vague characters of the nature of the source whence it was derived. Amongst other things we find, what was long suspected, but what M. Melloni first conclusively proved, that the presence or absence of light is, to a great extent, immaterial; no doubt a *concomitant*, but

which, when transmitted by any one of these, is found in a fitter state to pass through each of the others; and this modification is found to be more perceptible as the character of the heat is more removed from that which these media transmit most readily, that is, as the temperature of the source is higher. Thus heat derived from a lamp, has 36 per cent. transmitted by a certain smoked plate of rock-salt. But if the heat transmitted by the smoked salt has previously been sifted or analysed by transmission through another plate of smoked salt, through laminated mica, and through roughened salt, the per-centage is raised from 36 to 44 in the two former cases, and to  $40\frac{1}{2}$  in the latter, proving incontestably the specific action of these transmissions in arresting the more refrangible rays.

"I next considered, that as a moderate number of scratches appeared to produce this modification, it might be practicable to obtain the effect by transmitting heat simply through fine wire gauze. I could not obtain it finer than 60 wires to the inch, and, in this case, I could obtain no indications of differences in the transmitted ratios of one or other kind of heat. The proportion transmitted to the direct effect, was, in every case, almost exactly that of the area of the interstices of the gauze to its entire surface.

"When fine gratings (used for Fraunhofer's interference fringes) made of cotton-thread were used even in this case no difference was perceived; here, however, the thread, having probably a certain degree of permeability, might mask the effect.

"When fine powders were strewed between salt plates, leaving minute interstices, the easier transmission of heat of low temperature was again apparent.

"Having procured delicate lines to be drawn with a diamond point on a polished salt surface, first dividing it into squares 1—100th inch in the side, then into parallel stripes 1—200th inch apart, and finally into squares of the latter dimension, in every case the effect resembled that of random scratches, and was more apparent as the surface was more furrowed.

"I have finally to observe, that the mere process of natural tarnishing by the exposure of salt to the air, produces a similar effect.

"These facts evidently point to phenomena in heat, resembling diffraction and periodic colours in light. I cannot doubt that the simple transmission through

not an *indispensable* circumstance. Again certain relations had been established at an early period in the history of the science of heat, between the *colour* of a surface and the quantity of heat which it absorbed, and this relation for any two surfaces compared (as black and white, of similar textures), was first clearly shown by Sir John Leslie, to depend upon the luminosity of the source of heat, to which conceiving it proportional, that philosopher based upon it the principle of his photometer\*. Professor Powell, of Oxford, conceived and executed an ingenious experiment, by which it is demonstrated that the interposition of a screen of glass, though it stops but little light, alters most materially the influence of colour on the transmitted heat, thus annihilating at once the principle of photometric measurement adopted by Leslie, except in a very limited class of cases†. M. Melloni has fully confirmed the experiments of professor Powell‡, which therefore may be considered as establishing this conclusion, that the quality of *blackness* or *whiteness* of a surface affects its power of absorbing heat (*not* in proportion to the luminosity of that heat, as was formerly supposed, but) in proportion to its refrangibility.

4. It is both convenient and correct, therefore, to consider the refrangibility of heat as the cause of most of its distinctions of *kind* and *degree* of modification in our experiments, instead of making vague reference to the temperature of the source whence it is derived. Heat derived from the following scale of temperatures corresponds to heat of progressively elevated refrangibility; as, 1. Heat from ice has a less refrangibility than that from, 2. the hand, which again is below, 3. that from boiling-water: then comes, 4. that from a vessel of mercury under its boiling temperature, 5. a piece of smoked metal, heated by an alcohol lamp behind, but itself quite invisible in the dark; 6. incandescent platinum (a coil of wire in an alcohol flame), 7. an oil lamp (Locatelli's). Such is the scale of heat which has often been referred to in M. Melloni's researches and my own; but though our apprehension of the temperature of the source ceases to be so clear above this limit, and the colour and brightness of the light which accompanies the heat no longer varies distinguishably, the scale may fine metallic gratings would produce effects similar to those of the striated surfaces of rock-salt.—*December 16, 1839.*"

\* Essay on Heat, 1804.

† Phil. Trans. 1825, p. 187.

‡ *Ann. de Chimie*, April 1834. M. Melloni finds, for instance, that the rays from an oil-lamp falling on black and white surfaces, affects their temperature in the proportion of 1000: 805. And the same proportion holds if they be transmitted through a plate of rock-salt; but if a plate of alum be used, though equally transparent for light with the salt, the proportion is now 1000: 429.

be carried upwards indefinitely by interposing screens of different materials, which either may be proved *directly* (as I have done in the Third Series of these researches) to increase the refrangibility, or we may take Professor Powell's, or any similar test, which our experiments lead us to conclude to be co-ordinate with the fact of refrangibility. Such a prolongation of the scale of heat-sources would be,

8. Oil-lamp heat transmitted by Common mica.
9. \_\_\_\_\_ Glass (Argand lamp)
10. \_\_\_\_\_ Citric acid.
11. \_\_\_\_\_ Alum.
12. \_\_\_\_\_ Ice.

A clear appreciation of the scale of refrangibility as the important test for the qualities of heat cannot be too clearly apprehended and admitted. Heat from *any* source, if it admit of transmission at all through glass, alum, or water, will ultimately have the character of glass-heat, alum-heat, or water-heat, just as light from the sun, or from a candle, becomes red, blue, or green, by transmission through glasses of these colours.

5. Now, when M. Melloni had shown (and this experiment I believe was original to him), that substances which stop every ray of even intense light (as opaque glass and some kinds of dark mica), yet transmit a sensible quantity of heat, it was not unnatural to inquire whether the *invisible heat* thus obtained from a *luminous* source, might not possess the qualities of heat from a dark source, in other words, whether bodies, like black glass and mica, insted of stopping the less refrangible rays like glass, alum, &c., would not suffer these to escape, and absorb the most refrangible rays, acting upon heat as a body does upon light, which stops the yellow, blue, and violet rays, that is, as red glass does.

6. Experiment partly fulfils this expectation, and partly not. The careful and complete series of experiments made by M. Melloni upon the qualities of the invisible heat thus obtained\*, shows, that although it resembles low-temperature-heat, in so far as it is very feebly transmitted by alum or citric acid, yet low-temperature-heat (that from boiling water for instance) is but very faintly transmitted through the black glass or mica, which ought not to be the case if these bodies acted like a sieve, which arrested the more refrangible rays, and suffered the others to escape.

7 The direct test, however, of examining the refrangibility of the heat rays issuing from opaque screens yet remained; and

\* *Annales de Chimie*, April 1834.

in applying this, I proved that opaque glass and mica act as clear glass and mica do in *elevating the mean refrangibility of the transmitted heat*. Hence I concluded that the effect of such media upon heat is to absorb the rays of greatest and least refrangibility, in short, to act as homogeneous yellow glass would do upon light, the *mean* refrangibility being on the whole, however, increased by transmission. I also pointed out that heat from luminous sources is probably far more compound in its nature than dark heat; that the darkness of heat is no test of its refrangibility; and that even the most refrangible rays may contain heat separable from the light which accompanies it\*.

8. In all this, then, there appears nothing exactly equivalent to the action of red glass upon light,—no substance which transmits most easily heat of low refrangibility and temperature, and which separates heat of that description from the compound emanation from luminous sources. Reasoning probably upon the conclusions just stated, M. Melloni conceived the happy idea of combining an opaque substance, such as smoke, with a solid, which itself should effect no specific change upon the incident heat. He therefore smoked *rock-salt*, and found that it presented a complete analogy to red glass, transmitting most easily heat of low temperature and refrangibility.

9. Whilst I give full credit to M. Melloni for the ingenuity and importance of his experiment, I must be permitted to state, that I conceive that I preceded him by eighteen months in the discovery of a substance possessing similar properties, although I very readily admit, that, having been led to that observation incidentally, I first pursued the remark into consequences which I considered important, after M. Melloni had called particular attention to the experiment with smoked surfaces. On the 27th. February, 19th, and 20th. March 1838, (as appears by my Journal of Experiments,) I proved that mica, split into very thin films by the action of heat, such as I employ for polarizing, possesses the property of transmitting in larger proportion several of the less refrangible kinds of heat, and in particular, that it transmits heat from a source perfectly obscure, in almost exactly the same proportion with the highly refrangible heat of a lamp transmitted through glass. I have no hesitation in saying, that no other substance known previously to M. Melloni's experiments with smoked salt, gave any approximation to the following results, which are taken from the Third Series of my Researches, art. 24.

\* Researches on Heat. Third Series, art. 73, 81, &c.



*Table of the proportion of Heat from different sources transmitted by the Polarizing Mica Plates I. and K. contrasted with the transmissions of Mica in its usual state, and with Black Glass.*

Source of Heat.	Mica split by Heat. Plates I. and K.	Mica .015 inch thick.	Opake Black Glass.*
Locatelli Lamp, with glass ..	100	100	100
Locatelli .....	116	79	70
Incandescent Platinum .....	108	70	..
Brass at 700° .....	96	21	..
Heat at 212° .....	92	11	..

\* A contrast experiment made at the same time, March 20, 1838.

10. This singular result of the mechanical condition of the mica did not fail to strike me greatly at the time, and was not published until after careful repetition. It afforded a triumphant reply to an objection against my experiments which I was then combating, that the quantity of heat absorbed by the polarizing plates, had modified and even inverted the results, and having satisfied myself of that, I did not pursue the matter further. The moment, however, that I read M. Melloni's communication on Smoked Salt, I perceived the important light which the perfectly analagous case of the split Mica might throw upon the phenomenon. It was evident that the results were similar in kind, it was probable that they might be made to approximate in degree. Instead, therefore, of interposing Mica piles at the great and disadvantageous obliquities which I had employed (when I wished simply to test their action as polarizing plates) I took a split mica pile (frequently referred to in former parts of these Memoirs under the designation H.) and placed it *perpendicularly* to the incident rays of heat. I obtained the following results:—

Transmission through split Mica H. at a perpendicular Incidence.		
Source of Heat.	Per 100 of Incident Rays.	Relative Transmission.
Locatelli, with glass .....	9.2	100
Locatelli .....	13.7	150
Dark hot brass .....	17.3	188
Hot Water .....	16.3*	178

\* This observation having been made at a different time from the others, and probably not exactly under the same circumstances, I have stated it in the way least favourable to the views I entertain: the per-centage actually observed was 19.

11. It appears, then, very clearly, that this peculiar condi-

tion of mica induces, in opposition to the natural quality of the substance (9), the same peculiarity which a film of smoke possesses relatively to the incident heat. It is truly for heat what red glass is for light, it transmits most freely rays of lowest refrangibility.

12. Seeing clearly from the first that the change of character in mica was due to the splitting up into an almost infinite number of minute surfaces the natural laminæ of the mineral mica; and attributing the character of redness (so to speak) to the multiplied and irregular reflections and interferences which must so take place, it occurred to me as very probable, that the effect of smoke was due to the superposition of a prodigious number of minute opaque points upon a transparent surface, and *that* not so much from any physical peculiarity of its carbonaceous material, as from the mechanical distribution of opaque dust over the diaphragm of rock-salt.

13. This induced me to try the effect of *mechanical alterations of the physical surface* of the salt, expecting to find an effect analogous to that part of smoking, and, guided by no other grounds of conjecture than those which I have stated, I roughened with sand-paper both sides of a polished plate of rock-salt, furrowing each surface rectangulary until it was quite dim. I then examined its transmissive power for heat from different sources, and was gratified to find my anticipation realized. The proportion of dark heat transmitted, compared to that from a lamp sifted by glass, was no less than as 3 to 1 \*.

\* I state it as a proof of the conviction which I had of the real character of split mica with respect to heat, that the reasoning stated in the text was founded upon no experiments made subsequently to those of March 1838, already quoted. The very first entry in my journal-book of last autumn contains *simultaneous* experiments, (1.) on smoked salt, to verify M. Melloni's observations: (2.) on split mica, to extend my own of March 1838, to perpendicular incidences: (3.) on scratched surfaces, on the assumption that the two former would be realized. As M. Melloni thinks that I had not a clear idea of the properties of split mica, which, indeed, if I understand him, he still doubts, I will quote *verbatim* the passage in my laboratory-book alluded to.—"1839, Nov. 12. M. Melloni having lately stated (*Comptes Rendus*, 2nd Sept.) that smoked rock-salt is the only substance known which transmits heat of low temperature easier than luminous, this is in the first place contradicted by my experiments of 1838, Mar. 20, &c. on mica split by heat, already published,—and in the next place, I felt [feel] some doubt whether [in his experiments] it was the quality of the *material* or only the *surface* which affects the result. To try this, and to verify previous experiments, I smoked a plate of rock-salt; I *roughened* another with sand-paper, first on one, and then on both surfaces; I had also the split mica plate marked H placed *perpendicularly* to the rays of heat."

[*Here follow the Experiments.*]

"It clearly appears, then, that salt simply roughened transmits most dark heat. I presume that the effect of smoking is only superficial, and that roughening stifles luminous heat faster than dark heat."

This is the *first* entry in my book after the publication of M. Melloni's letter in the *Comptes Rendus*, and it is given *entire*.

14. It thus appeared that there are at least three conditions under which a medium can be found capable of transmitting heat of low refrangibility, and that two of these had reference *solely* to mechanical constitution. It was natural to generalize and attempt to include the case of the film of smoke, as well as the striated and the laminated surface, under one category. I have already said that the mechanical distribution of the opaque carbonaceous particles offered a plausible analogy, which I proceeded to attempt to carry out.

15. The numbers in art. 10, may be compared with the following:—

Source of Heat.	Transmission per 100 of Incident Rays, by		Relative Transmission by	
	Smoked Salt.	Rough Salt.	Smoked Salt.	Rough Salt.
Locatelli, with glass . . . .	30	49	100	100
Locatelli, . . . . .	—	62	—	126
Dark hot brass . . . . .	58	70	192	142
Hot water . . . . .	67	77	223	157

16. It occurred to me that if the action of the smoke was entirely a *superficial* one, or due to the character of a rough surface applied to the plate of rock-salt, that the effect of two such surfaces upon the transmission of heat would probably differ from that of a single film of smoke, so thick as to produce an equal absorption of heat of any particular degree of refrangibility. For this purpose I smoked three plates of polished rock-salt, so that two marked D and E absorbed *together* as much dark heat (very nearly) as the third plate A did alone.

17. I may take this opportunity of mentioning the way in which I have succeeded in smoking inflammable surfaces without burning them, or crystallized plates, like rock-salt, which crack and fly by the direct application of the flame of a candle. A coarse gas-flame, surrounded by a wide metal tube 10 or 15 inches long, against the side of which the flame partly plays, affords a stream of comparatively cool smoke, which may be applied to any given surface. With these three smoked salt-plates I obtained the following results:—

	Source of Heat.		
	Locatelli, with Glass.	Locatelli.	Dark Heat.
	Per Cent.	Per Cent.	Per Cent.
Smoked Salt Plate A . . . . .	8·3	17·2	32·9
D . . . . .	26	41	58
E . . . . .	23·5	36	53·5
(D + E) . . . . .	7·3	18	32·1

As most of these results are from single experiments, the first and the last line must be considered as almost identical, and certainly does not indicate any material specific difference in the absorbent qualities of one thick and two thin films of smoke, which might be expected, if the action were a merely superficial one.

18. From these numbers we deduce another conclusion of some importance. Since a film of smoke transmits most easily heat of low temperature and refragibility, we may expect that it will modify the quality of any compound beam of heat which it transmits, and that one such transmission will therefore render a second more easy. Now, we find that the plate D. transmitted 26 per cent of heat from the first of the above sources, and that of the 26 rays escaping from D. and falling upon a second smoked film E., E transmitted 7.3, or 28 per cent of those incident upon it. But by the third line of the table E. transmitted 23.5 per cent only of the direct rays, consequently the capacity for transmission has been increased. In the same way for Locatelli heat we find the percentage for E. raised from 36 to 44 by previous transmission through D; and for dark heat from 53.5 to 56.

19. Hence a useful application of smoked surfaces to which I have sometimes had recourse. It is often important to operate with more or less refrangible rays of heat under exactly the same circumstances of parallelism or divergence, and intensity. Having adjusted an Oil Lamp with a salt lens, so as to afford a compound-beam stronger than required, we may, by interposing a plate of smoked salt, absorb the most refrangible rays, and suffer the others alone to pass, and by then using a glass of proper thickness, the intensity of the heat may be reduced in the very same proportion, but the more refrangible (hottest) rays are alone retained\*.

20. Now the results of (17), though not what I anticipated as most probable, do not altogether relieve us from some doubt as to the nature of the action of the film of smoke, although those experiments, as well by others which are to be detailed in this paper, incline me to M. Melloni's opinion, that the

\* *Smoked glass* is evidently an excessively opaque compound medium, being composed of two parts which absorb opposite ends of the heat spectrum. It is curious to reflect how little the true cause of the opacity of a film of smoke deposited upon glass was understood at the time that it was quoted as a convincing proof of the *immediate* radiation of heat through solid bodies. Far from smoke being the untransparent substance supposed (I use the word loosely in applying it to heat), it transmits a quantity of some kinds of heat really surprising, although the thickness of the smoke be considerable.

smoke acts by its own intimate constitution, and not by its mechanical arrangement. Though I have examined smokey films with a powerful microscope, I have failed in detecting the minutely divided particles of carbonaceous matter of which it must undoubtedly consist. Still the reticulation which fine powder strewed on a surface must form, if it act by the minuteness of the spaces which are left (as in diffraction-experiments on light), must act more intensely when by superposition such reticulations become more minute and complicated. And it may little matter whether the smokey screens are distinct, and deposited on separate plates mechanically placed in succession, or whether they are accumulated by continued smoking on a single surface. I do not state this with a view to maintain my own original opinion, which I am rather disposed to abandon, and to consider a smoked surface, *diathermanous*, as well as *transparent*, in the full meaning of the words; but in extending my experiments to roughened surfaces, I was rather surprised to find that the continued action of furrowing the surface by scratching it with coarse sand-paper, not only diminished the transmission of heat, but increased the *specific* action on rays of different refrangibility, whilst one would rather have imagined that the action being here due to the destruction of polish, and therefore *superficial* any exaggeration of the roughness would not have increased the relative *diathermancy* to rays of low refrangibility.

21. Conclusive experiments, however, mark an increased sensibility to various kinds of heat by increased roughness. Two plates of salt, marked *a* and *b*, having been scored with sand-paper in rectangular directions on both sides, were placed so as to intercept similarly a parallel beam of heat. The difference of the following numbers is due to the less degree of roughness of *a*.

	Source of Heat.		
	Locatell, with Glass.	Locatelli.	Dark Hot Brass.
Rough Salt Plate <i>a</i> .....	Per cent. 30	Per cent. 48.5	Per cent. 59
<i>b</i> .....	16.6	28.5	45
<i>a + b</i> .....	7.2	16	27.5
Per-centage of heat } received through <i>a</i> } transmitted by <i>b</i> .. }	24	33	46.5
Ratio of <i>a</i> to <i>b</i> .....	160:55	100:58.5	100:76

Here, then, we find the per-centage of transmission raised in every case by a previous transmission through a rough surface. The increased facility of transmission is greater in proportion as the incident heat was more heterogeneous; dark heat undergoes very little change. It appears also by the last line of the table, that the increased roughness of *b* compared to *a*, had *enhanced* the characteristic effect (analogous to *redness* for light).

22. I have made a great many experiments to satisfy myself that the action of all the three media already specified (14) is precisely analogous, and that they actually insulate similar rays by absorption. The following table is a specimen, showing the increased facility with which rays of heat, from whatever source, are transmitted by smoked rock-salt after previous transmission through the same or other substances.

*Table showing the Per-centage of Transmission by the Smoked Rock-Salt Plate E for heat farom different sources, and modified by passing through the following Media.*

Source of Heat.	Heat transmitted by			
	Nothing.	Split Mica H.	Smoked Salt D	Rough Salt a
Locatelli, with glass . . . .	23·5	..	28	29
Locatelli . . . . .	36	43·5	44	40·4
Dark hot brass . . . . .	53·5	56	56	55

23. It is very important to consider how this action of rough surfaces may be explained, and whether we have any analogous phænomena in the case of light. Can it be owing to the circumstance that the depolished surface reflecting differently the various kinds of heat, those kinds least copiously reflected persevere, and form the majority of the transmitted rays? To this it may be replied, that the intensity of reflexion at polished surfaces is so insignificant at a perpendicular incidence for either heat or light\*, that were the *whole* specularly reflected heat, transmitted in the one case, and absorbed in the other, the difference, instead of amounting to 30 per cent. or more, of the incident heat [21], could not exceed 4 per cent.

24. Arguing from the analogous case of light, I anticipated on the contrary, that the *reflected* as well as the *transmitted* beam, would be more intense from such a surface, as it is well known that polish becomes more specular for rays of light con-

\* See Melloni, *Ann. de Chimie*, Dec. 1835, and my Memorandum on the Intensity of Reflected Heat and Light, Proceedings of the Royal Society of Edinburgh, p. 254,

sisting of longer undulations, the inequalities of the surface first becoming insignificant for red light.

25. In this I was not deceived. My purpose not being to investigate fully the subject of diffuse reflexion, I confined my attention to the establishment of the general fact. Employing an apparatus which I have not yet described, but which bears a great analogy to that figured in the Society's Transactions, vol. xiv. pl. xiii., and described in art. 51 of the Third Series, I observed the intensity of reflection of heat from different sources at a *single* polished surface of flint-glass, and at a similar surface depolished with emery. I obtained at considerable incidences the following striking results as to the increased susceptibility of heat to be *regularly* reflected at a rough surface, when it is of low temperature or refrangibility.

*Ratio of the intensities of Heat reflected by a Polished and a Rough Surface of Flint-Glass.*

Angle of Incidence.	Source of Heat.		
	Locatelli, with Glass	Loctalli.	Dark hot Brass
60°	..	100 : 34	100 : 35.4
70	100:26.5	100 : 38.3	100 : 43.5

So far then the character of the action of depolished surfaces is consistent. *The stifling effect* [ which diminishes both the reflected and refracted ray ] *of a rough or laminated surface diminishes with the refrangibility of the incident heat.* That the same thing takes place in the reflexion of light we know; it is probable that it does so in its transmission likewise, though this has not been so distinctly observed. Most impure substances transmit a ruddy gleam, vapour of water does so whenever it is not colourless\*, and every practical optician knows, that in a great majority of media the violet end of the spectrum is first absorbed.

26 A more minute analysis of the influence of surface upon Heat is what we now propose. And three questions present themselves for immediate solution: (1.) If deficiency of polish produce a variation in the proportion of not less than 3 to 1 in the quantity of transmitted radiated heat from different sources, can we employ Salt plates with the ordinary degree of polish, and yet consider them as equally transparent

\* Edinburgh Transactions, vol. xiv. p. 371.

for every kind of heat, as M. Melloni's discovery has hitherto entitled us to do? (2.) Is the effect of roughness common to other substances as well as rock-salt? (3.) The operation of de-polishing with sand-paper, is nothing more than the making of an infinite number of distinct grooves on a polished surface: supposing these grooves to be regularly formed, and capable of numerical estimation, will the effect continue?

27 (1.) With respect to the first of these questions, it is satisfactory to be able to answer it affirmatively in a general way. I took two salt plates, of which the surfaces had not been *regularly* polished for a long time, and which, though bright and clear, were by no means particularly even and true. Of heat from Locatelli's lamp previously sifted by glass, these *four surfaces* of rock-salt transmitted 72 per cent. With dark heat from smoked brass the per centage was 73, a difference which, in this experiment, could hardly be considered as appreciable. *The transmission of these two very different kinds of heat was therefore equal.* M. Melloni has shown that when rock-salt is pure and perfectly polished, .92 of the incident heat is transmitted by a pair of surfaces, and therefore four surfaces should transmit  $[\cdot 92]^2$  or 84.5 per cent. This estimate I have verified, and am satisfied of its accuracy. The deviation in the present case (which I think it right not to pass over) is due partly, no doubt, to the inequalities of surface, but chiefly owing to some imperfections in the salt itself, which as the experiment was merely a relative one, were not adverted to. In contrast with this, I used at the same time [December 11th. 1839] a piece of salt, which once had been polished on both sides; but which, by being laid aside for some years, had become completely dull and gray on its surface. This specimen, then, was simply *de-polished*; it contained no furrows, and had been subjected to no mechanical action whatever. Its per centage of transmission was,

	Locatelli, with Glass.	Dark hot Brass.
Tarnished salt .....	66	77

clearly establishing the general principle.

28. [2.] With respect to the question, whether roughness of surface had a similar effect in modifying the diathermancy of other substances as well as rock-salt, we are able to give a distinctly affirmative answer. Rock-salt being, so to speak, quite indifferent to the quality and source of the incident heat, any cause of specific action becomes immediately apparent. Not so with any other substance, which, exercising already a specific action by virtue of its nature, is to have that specific



action modified by a modification of surface. At least the question is, whether or not this modification will occur? An example will best illustrate how this modification may be discovered and expressed. I took a plate of Mica with its natural bright surfaces, and so thin as to transmit in considerable abundance heat from different sources. The per centages in this state were determined as follows:—

	Locatelli, with Glass.	Locatelli.	Dark Heat.
Mica with <i>bright</i> surfaces ...	83·5	74	37

Both sides of the Mica were de-polished with emery-paper, and the experiment repeated [27th. November, 1839]:

	Locatelli, with Glass.	Locatelli.	Dark Heat.
Mica with <i>rough</i> surfaces ...	45·5	51	31·5

Denoting the original transmissions by 100, the diminished effect due to the roughness of the surface will be represented by

54                  69                  85

demonstrating as clearly as possible that the stoppage is proportioned to the temperature of the source of heat; thus, whilst 46 per cent of the first kind was arrested by the roughness of the surface, only 15 per cent of dark heat was stopped.

29 [3.] With regard to the third question, the action of a comparatively small number of scratches on a polished surface, instead of a general diminution of its polish, I proceeded thus; I caused a series of extremely minute lines to be drawn mechanically with a diamond point, on a well polished surface of rock-salt, so as to divide it into squares having *one hundredth* of an inch for their side. A similar plate was scored by fine lines in the same manner, parallel to one another, and *one two-hundredth* of an inch apart. A portion of this second plate was crossed rectangularly, by lines drawn at the same distance, so as to divide the surface into squares four times smaller than in the first instance. These three media gave the following results, with two very different kinds of heat [December 6-11. 1839]:—

Source of Heat.	Scored in Squares 100 lines to the inch.	Scored in lines 200 to the inch	Scored in Squares 200 lines to the inch.
Locatelli, with Glass ..	76·5	61·5	45
Dark Hot Brass .....	82·3	61·5	64·5*

\* The part of the second plate which was scored *across* being more free from flaws than that which was *once* scored, explains the little difference between this number and that in the preceding column.

For heat of  $212^{\circ}$  the per-centage was still higher, as will afterwards be shown.

30. *Metallic Gratings*.—If the mere defect of transparency were the cause of the peculiar action of scratched surfaces, we might expect that any opaque filaments would act in the same way. Could we dispense with the *medium* altogether and employ a *screen*, which should have the qualities which we had artificially given to the physical surface of the medium, we should evidently have advanced a step in the interpretation of the phenomena. The action of grooved surfaces and gratings upon light suggested so forcible an analogy, that before I was able to procure the mechanically striated surfaces, described in the last article, I had employed fine metallic wire-gauze as a diffraction-screen, hoping to obtain results similar to those which I anticipated, and afterwards did obtain, by drawing fine lines upon rock-salt.

31. The fact that diffraction-phenomena in light, produced by gratings, are wholly irrespective of the nature of these gratings, as, for instance, whether they be formed of metal-wires, or mere lines drawn through a soapy film, stretched on glass, gave some countenance to this experiment. I was not unaware that diffraction spectra are produced, not by a parallel beam of light, but by a picture, formed of a distant luminous point. Still, though the ground or field illuminated by parallel rays passing through a grating must evidently have a uniform tint, it does not appear absurd to suppose that that tint may be different from white. Nor does this question appear to have occurred to mathematicians or optical writers, until the problem presented itself to me in the course of this investigation.

32. With such wire-gauze as I could easily procure, I failed in obtaining any peculiarity of action as relates to heat from different sources; and further, the quantity of heat intercepted by the metallic grating appeared to be nearly, or exactly proportional to the surface of the opaque portion of the screen. Thinking that perhaps finer gauze than that I used [60 wires to the inch] might produce the desired effect, I obtained, through the kind assistance of Sir John Robinson and M. Leonor Fresnel, the finest manufactured in Paris, going as high as about 160 per inch. In general my first views were confirmed, viz. [1.] that the proportion of heat stopped is irrespective of the source; [2.] that it is to the incident heat as the area of the wires is to the area of the surface. It must be observed, however, that the determination of this latter proportion with extreme accuracy by an examination of the grating, is not so easy as might at first sight appear. When the

wire is fine compared to the *interstices*, the interstices are pretty nearly rectangular and equal-sided. But this is not the case in most manufactured wire-gauze. One set of wires is nearly parallel and straight, but not so the set interlaced with the former, which do not generally make their intersections at right angles, and hence, universally the interstices are somewhat smaller than a calculation proceeding upon the number of wires per inch, and their diameter would give. Distrusting my own observations, I put three specimens of wire-gauze into Mr. John Adie's hands, requesting him to determine the mean diameters and intervals of the wires. With a very accurate Micrometer he determined 14 values for each of these quantities in both directions. From these data the proportion of the interstices to the whole surface of each grating is easily calculated, and the results are given below for three sorts of gauze, of which I had *previously* determined the permeability for heat.

*Micrometric Measurement of Wire-Gauze. Unit of Measure*  $= \frac{1}{52400}$  *inch.*

Wire-Gauze.	Lengthwise.		Breadthwise.		Ratio of interstices to surface.
	Interstice.	Wire.	Interstice.	Wire.	
No. 1. (57 per inch) ..	534	371	562	384	.3504
No. 2. (92 per inch) ..	375.6	179.4	402.6	179.6	.4680
No. 8. (129 per inch) ..	200	159	284	168	.3500

33 The numbers in the last column (computed on the supposition of the interstices being geometrical rectangles) are to be compared with the following experimental determinations of the proportion of incident heat transmitted by these gratings.

*Proportions per 100 of Incident Rays of Heat transmitted by Wire-Gauze.*

Wire-Gauze.	Locatelli, with Glass.	Locatelli.	Dark Hot Brass.	Hot Water
No. 1. (57) ....	32.5	32	33.5	
No. 2. (92) ....	46.0*	...	44.7†	
No. 3. (129) ..	30.5	..	30	29.7

\* Two such gratings super-imposed, so that the wires formed angles of 45° with one another, gave for the per centage of transmission 20.7. The square root of this, or effect due to each grating, is 45.5, or almost the same as the number in the text.

† Two super-imposed gratings gave 21.2 per cent, or 46 for each system separately.

The differences for each grating, perhaps, do not exceed the errors of experiment. In every case these numbers are *inferior* to the geometrical interstices; but what inclines me to think that this difference is due to the irregularities of figure of the gauze [including the effect of *flattening* of the wires, where they overlap, making the interstices obtuse-angled] is this: that No. 2. in which the wires were finer compared to the interstices than in others [the total interstices being one-third part larger in proportion], and the Gauze evidently far more regularly formed than in the other cases, the per centage transmitted differs very little from the geometrical gauge. I own, at the same time, that a difference of five per cent in No. 3. [which is evidently not due to an error of observation] seems to me barely accounted for by this remark.

**34 Thread Gratings.**—With gratings of fine cotton-threads  $\frac{1}{100}$  dth inch apart, used for showing Fraunhofer's Spectra, I obtained a similar result. These threads were arranged parallel-wise on two frames, capable of being super-imposed rectangularly. Thus, we can either employ a screen of parallel threads *one-hundredth* of an inch apart, or a screen of mathematically accurate squares, formed by super-position. It is difficult in this case, however, to obtain the diameter of the thread accurately enough to estimate the ratio of interstices.

*Per-centage of Incident Heat transmitted by Cotton Thread Gratings,  $\frac{1}{100}$  dth inch apart.*

	Locatelli, with Glass.	Dark Heat.
Third Grating, SINGLE .....	29.5	30.2
DOUBLE .....	9.0*	8.3†
* Corresponding <i>single</i> action = 30.0 per cent.		
† ————— = 28.8		

The difference here seems imperceptible, the differences, such as they are, being in opposite directions. The results in the last column are from single experiments (November 28th. 1839.)

**35 Action of powders.**—Adhering to the idea [12.] that the action of a smoked surface was due to the mechanical action of a number of minute opaque points distributed over a transparent body, it occurred to me almost at the commencement of these experiments, to try the effects of powders artificially sifted on such a surface. Any ingredient, however, which could make the Powders adhere to the surface,

would have vitiated the experiment, by introducing its own proper diathermancy. I therefore included the powders between two polished plates of rock-salt, closed at the edges with wax. The preliminary experiment (27.), to show that the salt surfaces, in the state in which I commonly employed them, exercise no perceptible influence on the quality of the transmitted heat, was evidently a very important one for the conclusions I meant to draw. It was, as I have stated, quite satisfactory.

36. The first experiments which I made with powders (December 6, 1839), were with chalk and alum, finely dusted between two plates of salt. I selected the chalk on account of its absolutely uncrystalline and opaque character; and alum, because its power of stopping rays of heat of low temperature was so very great, that I judged that if the influence as a *mechanical modifier of surface* should prove predominant, and allow as much, or more, heat of *low* than of *high* temperature to pass, the *mechanical* influence of a substance in fine powder would be clearly established.

37. Now, the result at which I arrived, and which was entirely conformable to my anticipation, may serve to show the caution requisite in drawing conclusions from limited data, however apparently conclusive. The surfaces powdered with chalk suffered *rather more* heat of low than of high temperature to pass (viz. 34·5 per cent. dark heat, and only 30·5 of heat from Locatelli lamp, transmitted through a thick glass-lens), whilst the salt strewed with alum appeared quite indifferent to the kind of heat incident\* (transmitting only 17 per cent. of both, thus showing that the powder was in considerable quantity). I concluded therefore with apparent reason, that the chalk having no *specific* action, or being (most probably) opaque or *athermanous*, the powder of it acting mechanically, allowed low temperature heat to pass in excess, whilst in the case of alum, the *specific* action was entirely counteracted by the *mechanical* action of the powder. I simply stated the fact amongst others detailed in the preceding pages, in a memorandum presented to the Royal Society of Edinburgh on the 16th December 1839 and a few days after, in a slightly different form, communicated to M. Arago, and printed in the *Comptes Rendus de l'Académie des Sciences*, 6th January 1840. On 28th of December, I obtained a similar result for charcoal powder (whose affinity with smoke

\* Yet an alum plate of a certain thickness transmits no less than 27 per cent. of the one kind of heat, and *no sensible portion* of the other (Melloni).

suggested its use), and yet it does not appear that the general conclusion which I intended is entirely warranted.

37. It is well known that Sir Isaac Newton overlooked the variable dispersive power of bodies for light, in consequence of having compared two, in which the dispersion *happened* to be proportional to the mean refraction. A similar haste to generalize would have led to error on the present occasion, had not a simultaneous investigation led me to re-consider the subject of powders. Whilst waiting for the arrival of fine wire-gauze from Paris, it occurred to me to try the effect of metals in a state of extreme division. It seemed, however, first desirable to ascertain whether the metals are as incapable of transmitting heat as is commonly supposed.

38. For this purpose, I stretched a piece of the thinnest gold-leaf across a wide diaphragm of pasteboard, and suffered an intense parallel beam of heat from Locatelli's lamp to fall directly upon the pile. A screen of glass was interposed, which, by experiment, was known to stop 43 per cent. of this sort of heat. The needle of the galvanometer deviated  $31^{\circ}2$ , the glass being interposed; the equivalent direct effect would have been  $31.2 \times \frac{100}{43} = 72^{\circ}$ . When the glass was removed and the gold-leaf put in its place, on the brass screen being alternately introduced and removed, not the faintest motion was perceptible in the needle; had it amounted to  $\frac{1}{20}$ th of a degree, that is, had  $\frac{1}{1400}$ dth of the incident heat been transmitted by the gold-leaf, I considered that the effect would have been perceptible. Yet this gold-leaf was so thin that the features of a landscape could be distinctly seen through it, of the usual bluish-green tint. No more convincing proof certainly can be desired, that conduction plays no sensible part in these experiments, since it did not sensibly act on a film of one of the best-known conductors of heat, and perhaps not more than  $\frac{1}{300000}$ dth of an inch thick. I thought it worth while to repeat the experiment with dark heat, and with the same results. The analogy of the action of split mica on light to metallic reflexion led me to suspect, that if any kind of heat were transmitted by metallic leaves, it would be that of low temperature.

39. The imperviousness to heat of gold-leaf, the thinnest continuous film of metal which we can obtain, satisfied me of the importance of obtaining the metals in a condition to verify my experiments with the powder of other substances. When the hope diminished of obtaining a wire-gauze of a de-

gree of fineness (I mean fineness in the *wire*, not closeness of texture, for that was comparatively immaterial) which might vie with the diamond scratches on the salt surface, which presented, under the Microscope, an irregular furrow, probably nearly  $\frac{1}{2000}$ th of an inch in mean breadth,—I resorted to the project of using the metals in *powder*. It was evident, from the experiments on de-polished and scored surfaces, that the *irregularity* of these streaks had nothing whatever to do with the phenomena of checked rays of high refrangibility, and admitting others. Sand-paper scratches, than which nothing can be more irregular, produced the effect, and that more intensely, as the surface became more coarsely and closely furrowed. Nay, it occurs in natural tarnish, where there can be no linear arrangement of the points effected. It seemed to me, therefore, that a surface covered with a metallic powder, presented the *limit* of a grating where the interstices were not required to have any regular form.

40. The next difficulty was to obtain impalpable Powders, indubitably metallic, to which I attached considerable importance, for it was quite conceivable that the metallic sulphurets, and other subjects employed for the fictitious metallic powders called gold, silver, and copper bronzes, might have specific diathermancies, which might injure the experiment. I at length succeeded in obtaining silver by precipitation, and copper from Daniell's battery: and with some difficulty I procured from a large manufacturer coinage silver and gold, reduced by mechanical trituration to a perfectly impalpable and beautifully metallic powder. These expensive preparations are now wholly superseded by the admirable fictitious bronzes in use in the Arts. These, together with metallic copper-bronze, perfectly impalpable, furnished by the same individual, and a much coarser Tin powder used by druggists, formed the material of a very careful series of experiments, which I extended over a very considerable period, and varied in a great many ways. [1840, JAN. 28. &c.]

41. The following table contains the results of my experiments on Metallic Powders, which (with the exception of tin) may be considered as perfectly impalpable, adhering to the dry finger, and *undoubtedly metallic*.

*Per-centage of Heat from different Sources transmitted by Metallic Powders.*

Powder.	Locatelli Lamp.			Dark Hot Brass.	Hot Water.	Remarks.
	Glass interposed.	Direct.	Smoked Salt interposed.			
Gold, No. 1, (adhering to a single surface of salt.)	58	..	50.5			{ Mean of a considerable number of results.
Gold, No. 2, (between two salt plates.)	7.4*	..	..	4.1*		
Silver, No. 1, (between two salt plates.)	25.3	24.2	..	21.8	..	
Silver, No. 2, (adhering to a single surface of salt.)						{ The same plate was used, but differently placed in respect of the pile, so that each series stands by itself
1st Series	27.7	..	18.5	..	..	
2nd Series	29.5	..	22.1	..	25	
Copper, No. 1, (between two salt plates.)						{ Mean of a considerable number of results.
1st Series	14.8	..	16.0			
2nd Series	17.4	..	..	18.7	17	
Copper, No. 2, (between two salt plates.)	5.6*	..	..	4.05*		{ Mean of a considerable number of results.
Tin (between two salt plates.)	27.0	26.0	..	25.5	..	

42 These observations are confessedly very imperfect. I am persuaded, however, that the apparent anomalies are not errors of observation: other instances will presently occur. With a view to determine the quality of thickly-strewed surfaces, yielding a very feeble per centage of transmitted heat, it was desirable to use an intense incident beam. In order, however, to keep the comparison within the range of galvanometer degrees, whose numerical values have been tested [Second Series, Arts. 7-8], the observations in the preceding table marked thus\* were made in the following manner. The *direct* effect of the incident heat on the pile was never observed, but only that part of it which penetrated the wire-gauze, No. 3. of Art. 33, which transmits almost exactly 30 per cent. of every kind of heat. The direct effect was estimated at  $\frac{100}{30}$  of the degrees of deviation corresponding to this transmission, and then the wire-gauze being removed, and the medium to be examined substituted the effect was compared to the computed direct effect. For example, with the copper powder, [No. 2.] the effect of the Lo-

\* See the next Article [42.]



catelli Lamp, heat transmitted through thick plate-glass, and then modified by wire-gauze, was .....  $22^{\circ}57$

Direct effect =  $22^{\circ}57 \times \frac{10}{3}$  .....  $75^{\circ}2$

Wire-gauze removed, and copper substituted .....  $4^{\circ}15$

Ratio to direct  $5^{\circ}52:100$

In this way per centages may be obtained with very nice accuracy: another experiment gave in the same case  $5^{\circ}50:100$ .

43. The Table in Art. 41. demonstrates to my conviction (strengthened by a careful examination of the very consistent observations on which it is founded), [1.] That gold, silver, and tin powders, instead of having the property which I was disposed to assign to opake powders generally, do really transmit more heat of low than of high temperature; that is, act like glass, alum, and other transparent media in their common state. [2.] With respect to copper, two series give one result, and a third the opposite. Yet all of these were made with great care, and contain internal evidence of their accuracy. I am confident that the differences are not due to errors of observation: and I have observed other cases, in which an increase of thickness of the obstructing medium, and an increased intensity of the incident heat, gave altered results as to a permeability, a result by no means paradoxical, since intense heat may be sensibly transmitted through a nearly opake substance, and thence acquire a new character, which a feebler beam, transmitted through a less obstructing medium, would not possess. At all events, I can offer no further explanation at present. That copper possesses a peculiar character, distinct from the other metals which I tried, I am fully persuaded.

44. The evidence which the experiments on Metallic Powders gave of the adequacy of the *mere powdery form* to produce the effect of smoke, forced me to a more critical examination of other bodies in a similar state.

45. I repeated my experiments with increased care on the powders already employed. I tried a great number of new ones, chosen amongst substances differing as widely in nature as possible. Some of these substances were repeatedly tried in different specimens, the powder more or less thickly strewed, and at different times.

46. One circumstance in particular raised a doubt as to the result of my former conclusion, where it seemed most incontrovertable. I had argued, that if alum in powder arrested equally all kinds of heat, the *mechanical* action of the powder must have opposed and destroyed the *specific* action of the

alum. [36.] I was gradually, however, led to admit, that, *in the state of powder, most diathermanous bodies are almost equally opaque*; or rather perhaps, I should say, equally indifferent to the *kind* of incident heat, *i. e.* colourless in optics.

47. So far as the eye could judge of the proportion of obstacles in surfaces strewed with different kinds of powders, there did not seem any very marked peculiarity in their transparency for heat. A surface dusted with alum or citric-acid, appeared to transmit nearly as much as one strewed with powdered rock-salt. Nor could this arise merely from the *minute thickness* of the substance, which is well known to produce in heat, as in light, an approximation to a colourless character; for the proportion stopped by the powder was always a large fraction (usually from  $\frac{3}{4}$ ths. to 9-10ths.) of the incident heat. The opacity, then, is the result of innumerable reflexions and interferences which *scatter* and *stifle* the transmitted heat; and this is almost equally effectually done, whatever be the nature of the substance. On reflection, therefore, this general result does not appear surprising. I will quote one experiment, in particular, in illustration of it.

48. When I was at a loss to procure fine metallic fibres, I thought of employing a diaphragm, irregularly covered with fine threads of spun glass, with a view (just as in the case of the alum powder) of ascertaining how far the mechanical condition of the glass might modify its well-known qualities with respect to the transmission of heat. When Locatelli Lamp-heat, having been transmitted by *thick plate glass*, fell upon the spun glass fibres, forming an irregularly reticulated diaphragm, no more than 47·5 per cent. of the incident heat was transmitted. Now, we know perfectly from the experiments of De la Roche and Melloni, that, after passing through such a thickness of plate glass, an additional film, the thickness of the glass fibres used would produce no *sensible resistance* to the further passage of the heat, excepting only its superficial reflexion. The loss of 52·5 per cent. of the heat was therefore due to the *scattering* and *stifling* of heat by reflection at the surfaces of the fibres, refraction through their cylindric surfaces, and interference. We cannot, therefore, be surprised, if the refracted part of the heat reaching the pile (the only portion very materially effected by the nature of the medium) should not greatly alter the quantity of different sorts of heat indicated by the Galvalometer. Accordingly we find, that heat from a dark surface of brass warmed by an Alcohol Lamp, had forty-four per cent. transmitted under the same circumstances: and even hot water had forty-two per cent.

although a small thickness of glass is sensibly opaque for that kind of heat.

49. If this be the case—if the difference be so trifling—for a reticulation of regularly formed, transparent, and polished threads of glass, much more must it hold with impalpable crystalline or other powders, presenting (no doubt) minute surfaces at every angle, and minute fissures in every direction.

50. The following table contains the results of a large number of experiments on powders of various kinds, many of them repeated under various circumstances. The investigation is, as in the case of the metallic powders, confessedly imperfect; but since the broad simple principle which I at first tried to establish respecting the diathermanous quality of opaque powder does not appear to hold universally, I stopped this series of experiments, which were troublesome and laborious, after establishing a few general facts, which I will presently lay down, without attempting to exhaust a subject, of which, by and by, we shall no doubt know more, but which at present it would be perhaps a waste of time to pursue into its insulated details. These powders were in all cases dusted between polished salt-plates, united at the edges, and then attached to diaphragms of card, so arranged as to transmit the heat in every case through the same parts of the surface.

*Percentage of Transmission of Heat, from different sources, through non-Metallic\* Powders.*

Powder of	Source of Heat.			
	Locatelli Lamp.		Dark Hot Brass	Hot Water.
	Through Glass.	Through Smoked Salt.		
Alum, No. 1. ....	17.0	..	17.1	..
— No. 2. ....	15.2*	..	13.0*	..
Citric Acid, No. 1. ....	29	30	33 †	31.5
— No. 2. ....	12.9*	..	8.7*	..
Rock Salt, No. 1. ....	12.8*	11.8	11.3	..
— No. 2. ....	31.5†	..	29.2†	..
Sulphur .....	50.0	..	44.7	..
Red Lead .....	30.2	..	34.0	..
Galena .....	26.3	22.4	..	..
Charcoal A. ....	5	..	9	..
— No. 1. 1st. Series § ....	11.4	13.9	..	..
— No. 2. 2nd. Series § ....	15.1	..	16.0	17
Chalk, No. 1. ....	3.2*	..	3.5*	..
— No. 2. ....	30.5	..	34.5	..
— No. 3. ....	15.5* 15.6	18.4	17.9	..
Carbonate of Magnesia .....	27.5	..	32.0	..
	8.3	2.6	..	..

\* The observations marked thus were made with a powerful beam of heat in the way described in Art. 42.

† Not directly comparable with the other two observations on the same line, and probably three or four per cent. too high.

‡ Extremely good observations.

|| The intensities very feeble.

§ The circumstances in these two series varied, so as to make the one not directly comparable with the other; but each is perfectly good.

\* By non-Metallic is meant, not in the state of a pure or uncombined metal.

51. On the preceding Table, I would observe, [1.] That the pulverized crystalline bodies, such as Rock-Salt, Alum, Citric-acid, and Sulphur, exhibit no decided tendency to transmit an excess of heat of low temperature, depending on their powdery form. The carefully repeated experiment with rock-salt, is on this point very conclusive, since its indifference as a substance to the quality of the heat which it transmits would at once leave the effect, if any, due to mechanical condition, apparent. It even very evidently appears in this state to transmit *less* freely heat of low than heat of high temperature. [2.] Galena, the crystallized sulphuret of Lead, in fine powder, appears to possess the qualities of gold, silver, and tin. (43.) [3.] Red-Lead, Charcoal, Chalk and Magnesia, all substances in an opaque *earthy* condition, appear certainly to transmit an excess of *dark heat*. I think it probable that this list might be extended to most bodies having a similar mechanical constitution.

53 These distinctions, I am well aware, leave the *causes* of the difference of character of powders, and the peculiarities of tarnished surfaces, nearly in the same obscurity as before. In particular, I cannot but regard it as being singular, that a surface covered with powdered salt has no analogy, but even opposite properties, to one of the same material mechanically furrowed.\* The contrariety of action of metallic powders to those of opaque earths, is as singular as it was unexpected. I have already stated, however, my doubt whether a complete investigation of the peculiarities of specific substances would, *at present*, reward the necessary labour. I have made experiments on a few fibrous substances, as paper and membrane, which I thought might very probably act as tarnished surfaces do. There is an approximation to this, as will be seen in the common cambric or tissue paper. In the kind of tracing paper employed (which is made in Paris I believe, under the name of *papier végétal*,) there is evidently some foreign matter introduced to produce the transparency, which modifies the transmission. A close reticulation of cotton fibres has already been shown to exercise no specific action. [34.] The following Table contains a few results not included in preceding ones, and illustrating in several substances the quality of *heat-colour*, which in this paper we have been considering.

\* To put this in the most clear point of view, I used and compared two such plates in the same experiment.

*Per-centage of Heat transmitted by several Bodies,*

Substance.	Source of Heat.		
	Locatelli Lamp, with glass.	Dark Hot Brass.	Hot Water.
Gold-beater's skin .....	60	28	..
Cambric or tissue paper .....	8·6	10·5	..
Tracing paper ( <i>Papier végétal</i> ) .....	36	28	..
Fibres of spun glass .....	47·5	44	42
Smoked salt .....	30·2	58	67
Roughened salt .....	49	73	76
Polished salt, scored into .... } 200 X 200 squares per inch. }	49·5	73	77

54. The leading facts contained in this paper are these:—

55. I. The peculiar (*red-like*) character of films of smoke in transmitting heat of low temperature is partaken,—

A. By simple powder of charcoal.

B. By [at least some] other dull earthy powders.

C. By surfaces simply *dull* or devoid of polish.

D. By surfaces irregularly furrowed, as with emery or sand-paper.

E. By polished surfaces, on which fine distinct lines have been drawn.

F. By the mechanical lamination of transparent mica, which, as a continuous medium, possesses opposite properties.

56. II. The following media seem *indifferent* to the kind of heat which they transmit:—

A. The thinnest gold-leaf is impervious to any.

B. Metallic gratings transmit all kinds of heat in a proportion which is probably exactly as the area of the interstices which they present.

C. Thread gratings.

D. In a state of powder, most crystalline bodies *approach* to a condition of opacity for heat.

57. III. The following bodies, in addition to those commonly known, transmit most heat of high temperature [*violet-like heat*].

A. Several pure metallic powders.

B. Rock-salt in powder; and many other powders.

C. Animal membrane.

58. IV. Heat of low temperature is most regularly reflected at imperfectly polished surfaces. It is also, we have seen, most regularly transmitted. These facts are of great importance to the theory of heat; and may probably sug-

gest inquiries of no small interest with regard to light, and especially the phænomena of absorption.

59. We have already [24.] noticed the analogy which the fact stated in the last article bears to the easier reflexion of red than violet light from depolished surfaces, and in that fact we find a confirmation in the application of the undulatory doctrine to heat, and of the opinion that the waves producing heat are longer in proportion as the temperature of the source is less. The phænomena of transmission are more obscure; they may be compared either to the diffraction, or to the absorption, of light.

69. The action of lines on polished surfaces, similar to those used in many diffraction experiments, led to the inquiry [31.] whether the mean colour of light transmitted by gratings was necessarily unchanged? The question does not seem to have occurred to any one to whom I have mentioned it; and though the most likely result would seem to be, that there should be no change, the grounds of such an *à priori* opinion do not appear absolutely conclusive. Professor Kelland, however, has, I believe, first succeeded in integrating the expression for the illumination of a screen placed behind a grating of any kind [see Airy's Mathematical Tracts, page 328] on which a plane wave falls, and he informs me, that in every case where the breadth of the interstices is any multiple of the breadth of the wires or opaque spaces, the intensity is the same as if there were a diaphragm equal in size to the sum of the interstices of the grating.

61. This result [which seems quite sufficiently general for our purpose] is so far confirmed by the absolute *indifference* of metallic gratings to the quality of the incident heat.

62. It remains, however, to be explained how furrowed surfaces can act, except by intercepting, as an opaque network would do, a part of the heat. I cannot give an explanation which appears full and satisfactory, but the condition of mica split into thin laminæ by heat, and producing the same effect, may serve to guide us, perhaps, to something like the true cause.

93. A number of thin plates, of *exactly uniform* thickness, would transmit a certain colour, and reflect the complementary one. If there be a great preponderance of plates approximating to a certain thickness, and if the disproportion of the lengths of the incident waves be great, a large proportion will be in like manner transmitted, and the remainder stifled or reflected. If this effect is not so frequently observed in bodies

mechanically separated into films as we might expect, this is owing to the small range of length of wave in the visible parts of the spectrum. A small variation in the thickness of the film transmits or annihilates, by interference, each colour of the spectrum in succession. If the waves of heat be much more heterogeneous (as I have already surmised) than those of light, such effects would be proportionably more sensible.

64. Possibly a grooved surface may be considered as presenting a number of polished surfaces, partially detached from the general surface, under small obliquities to the incident rays; and we may suppose that these rays, after separation by partial reflexion and refraction, re-unite, with unequal retardations, producing *first* a destructive effect upon the shorter waves, and suffering the others to persevere. I have already adverted to the fact, that most turbid fluids transmit chiefly the longer luminous waves. I offer these, however, but as vague conjectures upon a very obscure subject. I think that experiments on the colour of Media, such as those we have employed, and especially of de-polished plates, might not be without value in illustrating the phenomena of absorption in Optics.

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65 In conclusion, it might perhaps be expected that I should take some notice of the experiments and reasonings of which M. Melloni has addressed an account to M. Arago, in two Letters dated the 4th. and 14th. of March last, and published in the "*Comptes Rendus*" for the 30th. of the same month. These Letters were occasioned by the announcement of my Researches, in the same work, for the 6th. of January. The present paper, founded *solely* upon experiments undertaken and completed before the despatch of the earliest of M. Melloni's communications, will, I think, sufficiently answer all the questions which are started in his Letters to M. Arago, at least all those in which my experiments are concerned.

May 12th. 1849.

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XXV. *Notice on some peculiar Voltaic Arrangements.* By  
Dr. C. F. SCHÆNBEIN.\*

I am not aware that any philosopher has as yet pointed out the possibility of constructing voltaic arrangements which as to their mode of exciting current electricity must be con-

\* Phil. Mag.

sidered in some respects at least as the very reverse of what our ordinary hydro-electric circles are. That such association can be made will appear from what I am going to state. Having of late made some researches with the view of discovering the true cause of the polar state which the electrodes acquire within electrolytic fluids during the passage of a current, I was led to observe a striking analogy between metallic peroxides on one side, and chlorine and bromine on the other, with regard to the electromotive power of those substances, all of them being highly what is called electro-negative. First let us consider the way in which the two substances last mentioned give rise to currents. If an open glass tube being tied up at one of its extremities with a piece of bladder and holding an aqueous solution either of chlorine or bromine be put within a tumbler filled with water, and both fluids connected by the means of platina-wires with a delicate galvanometer, a continuous current makes its appearance, the direction of which is such, as to indicate that the fluid holding chlorine dissolved, is to pure water what copper is to zinc. How is this current excited? Does it perhaps originate in some action exerted by chlorine upon platina? I do not know whether a weak solution of chlorine has at common temperatures the power to act chemically upon that metal; I strongly doubt it: but let us suppose such to be the case, it appears to me that the current in question cannot be accounted for in the manner alluded to. If the current was due to some chemical action of chlorine upon platina, its direction ought, according to the theoretical views of some distinguished electricians, [to De la Rive and Becquerel's for instance] to be the very opposite to that which is really observed, the chemical union of chlorine with any metal being in a voltaic point of view considered by them as equivalent to the oxidation of metallic bodies. But it is much less the direction of the current in question which makes me think that it (the current) cannot be excited in consequence of the formation of some chloride of platina, than the fact that any current should be called forth under the circumstances mentioned. Agreeably to my views, and I think they will coincide with those of Faraday, no current at all would be excited within the arrangement spoken of if no electrolytic body were decomposed there, though chlorine should happen to act ever so powerfully upon platina; but if the current in question is not due to any action of chlorine upon the metal mentioned, we may ask, what is the chemical cause that disturbs the electrical equilibrium within our arrangements? Though I am



not aware that chemists allow chlorine or bromine to decompose water in any sensible degree without being assisted by the agency of light or heat, I am nevertheless prepared to think that such an action takes place, and that it is this which must be considered as the chemical cause of the current spoken of. According to my opinion, chlorine or bromine bears a similar relation to the hydrogen of water that distilled or amalgamated zinc does to the oxygen of (acidulated) water: in either cases there is no apparent chemical action, *i. e.* no decomposition of water previously to a current being formed or a path opened for a current. But no sooner has the circuit been completed in our two cases, than both the decomposition of water, and the formation either of muriatic acid or oxide of zinc, begins to take place, and a current makes its appearance. So far the two cases appear entirely similar to each other, but there is some essential difference between them which I will point out. In one case, chlorine is hydrogenized at the expense of the hydrogen of water, in the other zinc is oxidized at the expense of the oxygen of water; in one case oxygen is set free at the chlorine, in the other hydrogen is evolved at the zinc; or what comes to the same, in one case hydrogen is transferred from water to chlorine, in the other case oxygen is carried over from water to zinc. Now it is very easy to conceive that chemical actions being so diametrically opposite to each other as those in question obviously are, cannot produce the same electro-motive effect, in other terms cannot cause the electrolyzation of water in the same manner. Let us consider in what way water is electrolyzed in each of the cases under discussion. The particle of oxygen separated from one molecule of water by a particle of chlorine, unites with the hydrogen contained in the molecule of water which is contiguous to the oxygen disengaged, the oxygen of the latter molecule of water combines with the hydrogen of a third one, and so on, until the whole row of contiguous particles of water placed between the electrodes (the ends of the wire connecting the two fluids) have undergone a similar decomposition and recombination, and oxygen is at last disengaged at the positive electrode. The atom of hydrogen eliminated by the action of zinc from the molecule of water which is in immediate contact with that metal associates itself with the oxygen of a third molecule, and so on until an atom of hydrogen is evolved at the negative electrode. From what I have just now stated it appears that the particles of oxygen contained in the molecules of water approach zinc

whilst they recede from chlorine, or that the atoms of hydrogen move towards chlorine whilst they recede from zinc. If it be admitted that a current passing through an electrolytic body is made up by or is identical with a succession of chemical decompositions and recompositions which the molecules of the electrolyte undergo, we can easily conceive that what we term the direction of a current is determined by the direction in which one of the constituent parts of the Electrolyte moves with regard to the substance which chemically acts upon the Electrolyte. If the Electrolyte be water for instance, and its hydrogen happens to recede from the body that operates the composition of water, the current goes along with the hydrogen, and we call the body causing the chemical action electro-positive. If on the contrary, the oxygen of the Electrolyte recedes from the substance by means of which chemical action is brought about, the current approaches that substance, and we term the latter electro-negative. Generally speaking, the direction in which the atoms of hydrogen move during the electrolyzation of water, is also the direction in which the current circulates, both motions being one and the same thing. After what has been said it cannot surprise us that the chemical decomposition of water, caused by chlorine or bromine produces a voltaic effect exactly opposite to that which is obtained by the decomposition of the same Electrolyte brought about by the action of zinc. As to the peroxides, it cannot be doubted that their surplus of oxygen acts the same voltaic part with regard to water, as chlorine and bromine do in the cases which have just now been discussed; but how does it happen that oxygen, being in a combined state, exerts any tendency to unite with the hydrogen that is chemically associated with oxygen? I tried to answer this puzzling question some time ago in a Letter to the Editor of the Phil. Mag.: having however ascertained since some facts which do not well agree with my views developed in the paper alluded to, I am inclined to consider the latter as erroneous. As I think it to be a fundamental principle of the Theory of Galvanism, that any hydro-electric current, even the weakest one, is absolutely dependent upon real chemical action: in other terms, that hydro-electric currents and certain chemical actions are the same things, I am forced to admit that the decomposition of per-oxide of Lead for instance, which takes place when that substance constitutes a voltaic circle with (acidulated) water and platina, is an immediate action; that is to say, the immediate effect of the tendency of one proportion of oxygen contained in the per-oxide of Lead to unite with the hydrogen of water, is

just as immediate as the tendency of zinc to combine chemically with the oxygen of water. But such a supposition certainly implies something which seems to be altogether impossible, or at least highly improbable; namely, that different portions of oxygen being in different states of combination, are not to be considered as being absolutely identical with each other with regard to their voltaic action or properties.

A supposition of this kind sounds indeed very strangely, and I am afraid will not be much relished by the majority of chemists, so much the less that oxygen being in a free state seems to have no voltaic action at all upon water.\* Extraordinary as the matter in question may appear to be at the first sight, it appears less so, in my opinion at least, on a closer inspection of the case. Dumas has shown, and as far as I can understand the subject, in a very satisfactory manner, that chlorine is capable of losing altogether its ordinary chemical bearings, in replacing the hydrogen which is contained in acetic-acid. In chlor-acetic acid, the element named acts the part of hydrogen so perfectly well, that the compound last mentioned has all the essential properties of common acetic-acid; and chlorine is so much masked in chlor-acetic acid, that by means even of the most delicate tests, its presence there cannot be made perceptible. Now if chlorine, being contained in different compounds, can exist in states which in a chemical point of view must be considered as being diametrically opposite to each other, if chlorine be apt to lose its ordinary affinities and to acquire new ones, I ask why should oxygen, and other bodies also, not be capable of undergoing a similar change of chemical character, when they are caused to enter into certain combinations; and why should it be beyond the reach of possibility that the second atom of oxygen contained in the peroxides, differs as to its chemical functions from the oxygen contained in water, and that it is possessed of properties which make it to a certain degree similar to free chlorine, bromine, iodine, &c, with regard to its action upon the hydrogen of water? In support of the view that the same substance is capable of performing very different chemical functions, I cannot omit to allude to the beautiful and interesting researches of Professor Graham, on the constitution of Salts. That distinguished chemist

\* A solution of oxygen in (acidulated) water being separated by a piece of bladder from (acidulated) water containing no oxygen dissolved, did not cause an obvious deviation of the needle when both fluids were connected with a most delicate Galvanometer.

has rendered it highly probable, if not altogether certain, that water acts in the very same compound very different parts; and he has demonstrated besides, that substances differing in many respects very widely from that fluid may replace it, and take upon themselves its different functions. These phenomena of *matalepsy*, highly interesting as they are in a purely chemical point of view, appear to me to be still more so in a voltaic one, because they promise to offer the key to the comprehension of many voltaic facts which up to this present moment bear an anomalous character. Though we may take it as a certain fact that one portion of the oxygen of the peroxides, that chlorine, and bromine are bodies which act in the same manner upon water, we do not know for all that the way in which they excite a current; or rather we do not know why any of the three substances first named acquire, under certain circumstances, the power of decomposing water. According to the views of De la Rive and some other electricians, the cause of the currents produced by hydro-electric circles is chemical action. Convinced as I am of the general truth of the principle laid down by the philosopher of Geneva, I cannot help doubting the correctness of that doctrine of his according to which chemical action must always precede the appearance of a current. As far as I understand the chemical theory of galvanism alluded to, it cannot account for the disturbance of the electric equilibrium which takes place within a voltaic circle formed out of such substances as do not chemically act upon each other under ordinary circumstances. Amalgamated or distilled zinc not being attacked by acidulated water, peroxide of lead not being acted upon by nitric acid, bromine or chlorine not decomposing water, ought not, according to De la Rive's hypothesis, to excite any current if arranged so as to constitute with platina a voltaic circle. In asserting that the theory in question cannot account for the currents which are excited by the last-mentioned arrangements, I am well aware of what Mr. De la Rive has brought forward with the intention of vindicating the correctness of his views. He maintains, that in the cases above stated there is some feeble chemical action taking place, and that it is this action which must be considered as the cause of the initial current. This current being the effect of a chemical action, becomes according to the opinion of De la Rive, itself the cause of an additional current by effecting *electrolization*, &c. (*Rècherches sur la cause de l'Electricité Voltaïque*, 1836. p. 128.) Ingenious as these views may be, I cannot convince myself of their

truth, until Mr. De la Rive has given proofs more decisive than those are which he has hitherto offered in favour of his assertion, made either explicitly or implicitly, that it is only a film of hydrogen which protects amalgamated zinc against the action of acidulated water; that peroxide of Lead and nitric-acid do chemically act upon each other; that chlorine is capable of de-composing water in darkness, &c. But if no such chemical actions take place in the cases mentioned, and if nevertheless chemical action be the real cause of hydro-electric currents, how are we to account for the excited currents in question? We know as yet very little indeed of the nature of that force which causes what we call chemical actions, and we are far from being acquainted with all the circumstances and conditions which bodies must be placed in, in order to make them effect each other chemically. On account of that very ignorance of ours, it appears to me to be no hazardous or inadmissible conjecture if we suppose the possibility of certain compounds and single bodies acting chemically upon each other only when arranged together in a particular manner as to their juxta-position. Peroxide of Lead, aqueous nitric-acid, and platina, are bodies which, under ordinary circumstances, are perfectly indifferent to each other; but you have no sooner arranged them into what we call a voltaic circle, than the peroxide is de-composed, nitrate of Lead produced, and water electrolyzed: that is to say, the hydrogen of the latter compound caused to act upon the surplus oxygen of the peroxide; chlorine, water, and platina on one side; zinc, acidulated water, and platina on the other, are similarly circumstanced, no chemical reaction taking place between them unless put together so as to form a circle. I know well enough that the phenomena in question are interpreted by the voltaists in favour of the fundamental principle of their theory, and that the chemical actions just spoken of are considered by many Philosophers as effects of a current produced by mere contact. But there are many facts known in our days, which in my humble opinion at least, cannot be reconciled with the theory alluded to, and which prove that a current excited within an hydro-electric arrangement is entirely dependent upon the chemical action taking place there. I consider as one of these facts the relation which exists between the passage of a current through an Electrolyte, and the de-composition of the latter. Dr. Faraday's beautiful researches have thrown a strong light on the mutual dependence of the two phenomena; that celebrated Philosopher does however maintain that feeble currents can pass through elec-

trolytic bodies without causing de-composition. I ascertained some time ago the important fact, that electrolyzation and the passing of a current are so absolutely dependent on each other, that not even the weakest current can go through any Electrolyte without de-composing the latter.\* Now if such be the case, are we not allowed to conclude, at least to conjecture, that Electrolyzation, *i. e.* chemical action of a specific kind and voltaic electricity are the same things; in other terms, that what we call an hydro-electric current is but a particular motion of the elementary molecules of an Electrolyte? There is another fact, which in my opinion, bears strongly upon the subject in question. As far as my knowledge goes, a *conditio, sine qua non*, for constructing an efficacious hydro-electric arrangement, is, that one of its component parts be a chemical compound of a certain kind, *i. e.* an Electrolyte. Placing myself upon the ground occupied by the strict voltaists, I cannot see the necessity of such a condition; and I am unable to conceive why simple bodies of themselves (without the interference of electrolytes) should not be capable of producing currents. But if I adopt the principles of the chemical Theory of Galvanism; and if besides I consider the hydro-electric current as a particular chemical motion of the elementary molecules of the electrolytic compound, I can understand well enough why an Electrolyte constitutes an indispensable part of an hydro-electric arrangement, and why for instance a pair of zinc and platina being plunged into mercury is incapable of exciting current electricity, although the latter metal chemically acts upon zinc, and though the three bodies mentioned are arranged after the manner of a voltaic circle. According to the views which I now hold, and to the adoption of which I have been led by my late researches, I am very far from being prepared to allow (as I formerly did in accordance with De la Rive) to any sort of chemical action the power of causing the phenomena of a current. It is only the de-composition of an Electrolyte, which I consider as the source of hydro-electric currents, and it is for that reason why I do not believe that nitric acid, for instance, produces any current within a voltaic arrangement by acting chemically upon some metallic parts of the latter. I hope to be able to publish before long a series

\* I perceive from No. 20. of the *Comptes Rendus*, that Mr. W. R. Grove has made a communication to the French Academy, in which there is stated the same fact with regard to water. In giving it, as a novel one, Mr. Grove was most likely not aware of what I published six months ago in the *Bibliothèque Univ.* No. 35. p. 169.: and in the *Lond. & Edin. Phil. Mag.* No. 85. p. 46.

of results, obtained from researches which I recently made for the purpose, to prove the correctness of my views. For the present I deem it sufficient to state one fact in support of them. Two cups are filled, one with chemically pure nitric-acid of ordinary strength, the other with an aqueous solution of potash, (being entirely free from air) and the two cups connected with one another by means of a platina wire. If now a copper wire be put in the acid fluid, a zinc wire in the alkaline solution, and the free ends of both wires made to communicate with the Galvanometer, a current makes its appearance, the direction of which is such as to show that zinc is positive to copper. The same result is always obtained whatever may be the degree of dilution of either fluid made use of. As far as I know, chemists do not allow pure zinc to be in the least chemically effected by an aqueous solution of chemically pure potash, whilst copper is readily dissolved by nitric acid. Now according to the views of De la Rive the copper wire in the arrangement before mentioned ought to be positive with regard to the zinc, which is immersed in the alkaline fluid. The Philosopher of Geneva has tried to reconcile some similar facts disagreeing with this Theory by asserting, that the largest portion of electricities separated from each other by the action of nitric acid upon some metal, re-unites upon the surface of the latter, on account of the great conductive force of the nitric acid (see the above mentioned Memoir, p. 39.) But it appears to me that such an explication cannot be applied at all to the case in question, because the alkaline solution and zinc of themselves (without making part of a closed circuit) do not chemically act upon each other; consequently cannot (according to the notions of De la Rive) produce any current, and because that metal continues to be positive to copper, though the acid into which the latter (the copper) plunges be ever so much diluted with water, that is to say, its conductive force ever so much diminished and brought even below that of the alkaline solution. Before I leave the subject in question, I cannot omit in a general way to state that the result of my recent Researches seem to prove that the voltaic relation which any two metals bear one to another within pure water is not changed at all, if the latter substance be mixed in any proportion with sulphuric, nitric, muriatic acid, potash, ammonia, &c. and that portions of the same metal, though they are plunged into very different aqueous fluids, and are very differently acted upon by the latter, cannot assume opposite voltaic conditions. For instance, if copper be negative to tin within water, these metals

continue to preserve that voltaic relation to one another whether they be placed within acidulated water, or within aqueous ammonia. If copper be negative to Lead within water, this voltaic bearing is not changed by putting the metals mentioned either into strong or into much diluted nitric acid. If one piece of zinc be plunged into a solution of potash, another in water mixed with sulphuric acid, muriatic acid, &c., the two pieces of zinc do not give rise to any current. I am well aware that these assertions are strongly at variance with the results of Davy, De la Rive, and others, and indeed also with what very simple experiments seem to show; but I am prepared to say, that if the same two metals are sometimes positive, sometimes negative with regard to each other, according to the chemical nature of the fluid into which they are plunged, this change of voltaic character is only apparent and not real, and that it is dependent upon some secondary circumstances, which occasion a modification of the surfaces of the metals employed. Having above ventured to start the idea, that many substances, being under ordinary circumstances altogether chemically indifferent to each other, might become capable of acting upon one another when arranged together in a peculiar manner, and presuming that such a conjecture will be considered by the majority of chemical philosophers as very extravagant and wild, I am desirous to say a few words more on the subject. We know now-a-days a series of chemical phenomena called forth not by what we term "affinity," but by the mere presence of certain bodies by what the French call "*action de présence*," Berzelius, "*force catalytique*." The de-composition of peroxide of hydrogen caused by the precious metals and by their oxides; the transformation of alcohol into acetal, aldehyde, and acetic acid brought about by the joint action of platina and oxygen; the well known union of oxygen with hydrogen effected by platina,\* are instances of chemical actions occasioned by a force which is altogether unknown to us, and widely different from what we conceive common affinity to be. Now if a substance can cause either the union of bodies with one another, or the de-composition of compounds, without entering into any combination with them, why, I ask, should the case be impossible, that certain substances do only chemically act upon each other, in consequence of their being put in contact with one other in

\* I have reasons to doubt the correctness of the very ingenious account which Dr. FARADAY has given of the phenomena in question, and shall not be long in making them known. I also consider the view taken by MR. DE LA RIVE on the subject as erroneous.



a peculiar way? For myself, I do not see any reason why such a thing should be impossible: at any rate, the fact that in many instances bodies do chemically act upon each other, merely as far as they are arranged in the shape of a circuit, cannot be denied any longer. The question is only how the fact is to be interpreted. Now if we consider the current excited in such cases as being due to chemical action, it seems to me that we cannot avoid arriving at a conclusion like that I have come to.

Bâle, July, 1839.

*Phil. Mag. S. 3. Vol. 15. No. 94. Aug. 1839.*

### *Antarctic Expedition.\**

[ We have read with much interest the recent account of Capt. Ross's proceedings, and we admire the resolution and fortitude with which he encountered and overcame the formidable dangers and difficulties which opposed his progress, and which had, apparently, completely stopped both the French and the American ships. Great and glorious as have been the military achievements of the British Navy, they will not be looked upon by posterity with more admiration than the extensive discoveries which have distinguished it in the present age—Science and civilization, and British power, have been promoted as effectually, and as permanently by the discoveries of Cook, and Parry, and Ross, and many others, as by the heroic deeds of Howe, and St. Vincent, and Nelson, and their glorious companions. The Admiralty have always most liberally and most patriotically encouraged those voyages of discovery; and in so doing they have consulted the honor of the nation, and honor is the most valuable of all national possessions.]

The Erebus and Terror discovery ships sailed from England about eighteen months ago, under the command of Capt. James Clarke Ross, and Com. Crozier, their main and ostensible object being to ascertain the true position of the South Magnetic Pole, and the exploration of the Antarctic Regions of which a very imperfect knowledge has hitherto been obtained; but a series of magnetic observations were also directed to be made at different stations on their route, and the first of which being at Maderia, they put in there and stopped several days. Thence they proceeded to St. Helena and the Cape of Good Hope, at which places they fitted up observatories, and left officers of sufficient scientific acquirements to superintend them.

Kerguelen Island was the next spot they visited, where, and at Sabria, further observations on the magnet were made and they arrived safe at Hobarton, Van Dieman Land, about the middle of last August. There Capt. Ross met his old

\* Nautical Magazine.

friend Sir John Franklin, the Governor, from whom he received every possible attention and assistance, and there also having erected and fitted up an observatory, they proceeded on their voyage about the 26th of October, that being the date of their last letters, at which time they were on the eve of their departure.

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*Extract of a letter from Capt. Ross of H. M. S. Erebus, dated at Hobarton, Van Deman Land, 7th April, 1841.*

"Under all circumstances, it appeared to me that, it would conduce more to the advancement of that branch of science, for which this expedition has been more specially sent forth, as well as for the extension of our geographical knowledge of the Antarctic Regions, to endeavour to penetrate to the southward, on about the 170th degree of east longitude by which the isodynamic oval, and the point exactly between the two foci of great magnetic intensity might be passed over and determined, and directly between the tracks of the Russian navigator, Bellingshausen, and our own Capt. James Cook, and after entering the Antarctic circle, to steer south-westerly towards the Pole, rather than attempt to approach it directly from the north on the unsuccessful footsteps of my predecessors.

"Accordingly on leaving Auckland Islands on the 12th of December, we proceeded to the southward, touching for a few days at Campbell Island, for magnetic purposes, and after passing amongst many icebergs to the southward of 63° latitude, we made the pack-edge, and entered the Antarctic circle on the 1st of January, 1841.

"This pack presented none of those formidable characters which I had been led to expect from the accounts of the Americans and French; but the circumstances were sufficiently unfavorable to deter me from entering it at this time, and a gale from the northward interrupted our operations for three or four days.

"On the 5th of January, we again made the pack about 100 miles to eastward in the latitude of 66° 45' S., and longitude 170° 16' E.; and although the wind was blowing directly on it, with a high sea running, we succeeded in entering it without either of the ships sustaining any injury; and after penetrating a few miles we were enabled to make our way to the southward with comparative ease and safety.

"On the following three or four days our progress was rendered more difficult and tedious, by thick fogs, light winds,

a heavy swell, and almost constant snow-showers; but a strong *water sky* to the south-east, which was seen at every interval of clear weather, encouraged us to persevere in that direction, and on the morning of the 9th, after sailing more than 200 miles through this pack, we gained a perfectly clear sea, and bore away south-west towards the Magnetic Pole.

On the morning of the 11th of January, when in latitude  $70^{\circ} 41' S.$ , and longitude  $172^{\circ} 36'$ , land was discovered at the distance, as it afterwards proved, of nearly a hundred miles directly in the course we were steering, and therefore directly between us and the Pole.

Although this circumstance was viewed at the time with considerable regret, as being likely to defeat one of the more important objects of the expedition, yet, it restored to England the honor of the discovery of the southernmost known land, which had been nobly won, and for more than twenty years possessed by Russia.

"Continuing our course towards this land, for many hours, we seemed scarcely to approach it, it rose in lofty mountain peaks of from 9 to 12000 feet in height, perfectly covered with eternal snow; the glaciers that descended from the mountain summit projected many miles into the ocean, and presented a perpendicular face of lofty cliffs. As we neared the land, some exposed patches of rock appeared; and steering towards a small bay for the purpose of effecting a landing, we found the shore so thickly lined for some miles with bergs and pack ice, and with a heavy swell dashing against it, we were obliged to abandon our purpose, and steer towards a more promising looking point to the south, off which we observed several small islands; and on the morning of the 12th, I landed, accompanied by Commander Crozier and a number of the officers of each ship, and took possession of the country in the name of Her Most Gracious Majesty Queen Victoria.

"The Island on which we landed is composed wholly of igneous rocks, numerous specimens of which, with other imbedded minerals were procured: it is in latitude  $71^{\circ} 56' S.$ , and longitude  $171^{\circ} 7' E.$

"Observing that the east coast of the main land tended to the southward, whilst the north shore took a north-westerly direction, I was led to hope that by penetrating to the south as far as practicable, it might be possible to pass beyond the Magnetic Pole, which our combined observations placed in  $76^{\circ}$  nearly; and thence, by steering westward, complete its circum-navigation. We accordingly pursued our course along

this magnificent land, and on the 23rd. of January, we reached  $74^{\circ} 15' S.$ , the highest southern latitude that had ever been attained by any preceding Navigators, and that by our own countryman, Capt. J. Weddell.

"Although greatly impeded by strong southerly gales, thick fogs and constant snow storms, we continued the examination of the coast to the southward, and on the 27th. we again landed on an Island in latitude  $76^{\circ} 8' S.$ , and longitude  $168^{\circ} 12' E.$  composed, as on the former occasion, entirely of igneous rocks.

"Still steering to the southward, early the next morning, a mountain of 12,400 feet above the level of the sea, was seen emitting flame and smoke in endless profusion.

"This magnificent volcano received the name of Mount Erebus. It is in latitude  $77^{\circ} 32' S.$ , and longitude  $167^{\circ} 0' E.$

"An extinct crater to the eastward of Mount Erebus, of somewhat less elevation, was called Mount Terror.

"The main land preserved its southerly trending, and we continued to follow it until, in the afternoon, when close in with the land, our further progress in that direction was prevented by a barrier of ice, stretching away from a projecting cape of the coast, directly to the E.S.E.

"This extraordinary barrier presented a perpendicular face of at least 150 feet, rising of course, far above the mast-heads of our ships, and completely concealing from our view every thing beyond it, except only the tops of a range of very lofty mountains in a S.S.E. direction, and in latitude  $79^{\circ}$  south.

"Pursuing the examination of this splendid barrier to the eastward, we reached the latitude of  $78^{\circ} 4' S.$ , the highest we were at any time able to attain, on the 2nd. of February; and having on the 9th. traced its continuity to the longitude of  $191^{\circ} 23'$  in latitude  $78^{\circ} S.$ , a distance of more than 300 miles, our further progress was prevented by a heavy pack, pressed closely against the barrier; and the narrow lane of water, by means of which we had penetrated thus far, became so completely covered by rapidly forming ice, that nothing but the strong breeze with which we were favoured enabled us to retrace our steps. When at a distance of less than half a mile from its lofty icy cliffs, we had soundings with 318 fathoms, on a bed of soft blue mud.

"With a temperature of  $20^{\circ}$  below the freezing point, we found the ice to form so rapidly on the surface, that any further examination of the barrier in so extremely severe a period of the season being impracticable, we stood away to the westward for the purpose of making another attempt to approach the Magnetic Pole, and again reached its latitude  $78^{\circ} S.$ , on

the 15th. of February, and although we found that much of the heavy ice had drifted away since our former attempt, and in its place, in a great measure, supplied by recent ice; yet we made some way through it, and got a few miles nearer that Pole than we had before been able to accomplish, when the heavy pack again frustrated all our efforts, completely filling the space of fifteen or sixteen miles between us and the shore. We were this time in latitude  $76^{\circ} 12' S.$ , and longitude  $164^{\circ}$ , the dip being  $88^{\circ} 40'$ , and variation  $109^{\circ} 24' E.$  We were, of course, 160 miles from the Magnetic Pole.

“Had it been possible to have approached any part of this coast, and to have found a place of security for the ships, we might have travelled this short distance over the land, but this proved to be utterly impracticable, and although our hopes of complete attainment have not been realized, it is some satisfaction to feel assured, that we have approached the magnetic Pole more nearly by some hundreds of miles than any of our predecessors, and from the multitude of observations that have been made in both ships, and in so many different directions from it, its position can be determined with nearly as much accuracy as if we had actually reached the spot itself.

“It had ever been an object of anxious desire with us to find a harbour for the ships, so as to enable us to make simultaneous observations with the numerous observers that would be at work on the important term-day of the 28th of February, as well as for other scientific purposes, but every part of the coast where indentations appeared, and where harbours on other shores usually occur, we found so perfectly filled with perennial ice of many hundred feet in thickness, that all our endeavours to find a place of shelter for our vessels were quite unavailing.

“Having now completed all that it appeared to me possible to accomplish in so high a latitude. at so advanced a period of the season, and desirous to obtain as much information as possible of the extent and form of the coast we had discovered, as also to guide, in some measure, our future operations, I bore away on the 18th of February for the north part of this land, and which by favor of a strong southerly gale, we reached on the morning of the 21st.

“We again endeavoured to effect a landing on this part of the coast, and were again defeated in our attempt by the heavy pack which extended for many miles from the above, and rendered it impossible.

"For several days we continued to examine the coast to the west-ward, tracing the pack edge along, until on the 25th of February we found the land abruptly to terminate in latitude  $70^{\circ} 40' S.$ , and longitude  $165^{\circ} E.$ , trending considerably to the southward of west, and presenting to our view an immense space occupied by the newly-formed ice, and so covered by recent snow, as to present the appearance of one unbroken mass, and defying any attempt to penetrate it.

"The great southern land we had discovered, and whose continuity we have traced from nearly the 70th. to the 79th degree of latitude, I am desirous to distinguish by the name of our Most Gracious Sovereign, QUEEN VICTORIA."

*Letter to M. Berthollet on the Magnetic State of Bodies which transmit a current of Electricity.* By MR. J. BERZELIUS.\*

I am engaged with some experiments to verify the beautiful discovery of M. Ørsted, relative to the influence exercised by the metallic wire which discharges the voltaic pile, over the magnetic needle. It is an extremely interesting thing to see electricity, light, caloric, and even magnetism, produced by the same cause and at the same time. Will it one day be possible to discover what all this phenomena means?

There is something mysterious in the expositions that are made concerning the magnetic phenomena of a wire conductor: however these phenomena are easily conceived when you consider the magnetic state of the conductor—a consideration that has been the object of my experiments, of which I am going to communicate to you the result.

It is known that bodies which possess magnetic virtue are in a state of polarity which generally accords with the length of the body,—but how is it conceived that in a fine wire magnetized in the way of its width and which appears to change its polarity according as the magnetic needle is placed above or below it?

In the neighbourhood of a Magnetic wire-conductor, the Magnetic Needle is placed under the influence of two forces; that of the wire become magnetized, and the magnetism of the Earth: the direction that it takes becomes, therefore, the result of the common action of these two forces. M. Ørsted has shown that the ordinary direction of the Magnetic Needle remains without alteration when the wire-conductor makes

\* We have given this, and the following, which show the opinions of their illustrious authors.—[Letter by the request of a correspondent.] EDIT.

an angle of  $90^\circ$  with the axis of the needle:—it is because, at this time the magnetism of the current and that of the Earth act in the same way; the Magnetic Needle would always assume this position relatively to the conductor, if the magnetism of the Earth did not exist. It follows, then, that the tension or magnetic polarity makes right angles with the electric current.

The following experiments have been made with a single pair of zinc and copper. The plate of zinc was a square of twelve inches, which was plunged into a trough of copper one inch wide. For a liquid conductor I used a mixture of sixty parts water to one of sulphuric-acid.—The wire-conductor made the Magnetic Needle deviate about  $25^\circ$

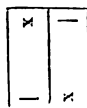
I have substituted for the wire a very fine leaf of Tin flattened to eight inches long by two inches wide, of which I have marked the middle with a longitudinal stroke. I have extended this leaf in the direction of the meridian, and in a plane vertical to the horizon. Near this leaf I have placed my Magnetic Needle suspended on a pivot, which I can raise and lower at pleasure, without giving any motion to the needle. I afterwards examined what happened to the needle when I connected the zinc and copper by means of this leaf; lowered towards the bottom edge of the tin foil, the needle was repulsed  $20^\circ$  from the magnetic meridian. By raising it gently towards the upper edge, this declination diminished until the needle had regained its first position parallel to the magnetic meridian. It was then in the same horizontal plane as the line of the middle of the foil; but the needle, inclined as was the case also in M. CErsted's experiment, when the Magnetic Needle was placed in the same horizontal plane with the wire-conductor. In continuing to raise the needle towards the upper edge of the foil, it was drawn until it declined  $20^\circ$  on the other side of the meridian—beyond the edge of the foil the declination diminished fast enough. I partly detached a piece of the upper edge of the foil and turned it up, so as to form a point raised about half an inch above the edge. I afterwards raised the needle, having one of its poles near this point; the declination that the needle showed, at first, was preserved as long as it was placed near the raised piece; it was visibly less at the same distance from the united edge. I afterwards made the electric current pass by a square of flattened tin, in the direction of two opposite angles. In examining, by means of the needle, the magnetic state of this square, it was found that the two opposite angles had become polar, and that the greatest declination took place at the two points—the larger the square, the more the magnetic intensity decreased: but it is

always in the opposite points the most distant from the conductor, that the declination is at its *maximum*, a circumstance which proves that the magnetic polarity of the current seeks the opposite extremities, just as it is the case in electric polarity and in that of artificial magnets.

All these phenomena would have taken place if I had used instead of the tin foil an artificial magnet, fine and wider than long. But the foil presented yet one phenomena that the magnet would not have produced, that is, that the same effects passed to the same degree on the other side of the foil, the upper edge, which on the side of the east attracted the south pole, repulsed it on the west side. The ordinary magnet would have attracted the same pole on the two sides. The magnetic phenomena of the foil take place then, as if they had applied two very fine and equally powerful magnets against one another, and in an inverse direction.

I repeated the experiment with the tin foil rendering its width parallel to the horizon, and keeping always the direction parallel to the magnetic meridian. In this position it acted on the needle exactly as a wire. The declination of the needle was at its *maximum* when its centre of rotation was in the middle of the foil. It is evident that a needle which is moved in a horizontal plane will not mark any line of magnetic culmination on a horizontal surface,—but in putting in equilibrio a needle which is moved in a vertical plane, and in making it pass under the magnetic surface from one side to the other, I find that the magnetic needle remains in equilibrio at the middle of the surface, and that at the edges attract and repel each its pole.

By these experiments we conceive what ought to be the magnetic state of a metallic parallelipiped, through which the electric current passes—each of its edges is a magnetic pole of which the breadth is equal to the length of the parallelipiped, or more exactly to the dimensions of the parallelipiped through which the electric current runs—the edges diametrically opposite contain the same kind of polarity, while those that terminate the same face contain an opposite polarity. We can then represent the interior magnetic state of a transverse section of the conductor, by two magnets with their poles opposed to one another nearly as the annexed figure represents it





It appears that each of the two electricities ought to be represented by its magnet, and that each has its analogous magnetic pole turned towards the same side with regard to its direction.

If for the parallelepiped we substitute a cylinder the magnetic state of the latter will be the same; but the magnetic phenomena will be more difficult to examine under this form: it is however that with which they have formerly studied them.

It appears to me probable that a cube through which an electric current passes might present some particular magnetic distribution—but by placing one of the edges of a metallic cube parallel to the magnetic meridian and by making an electric current pass in that direction through the cube, I have found that a magnetic needle placed under the lower surface has declined as commonly—a proof that a cube in this respect is only a very short parallelepiped.

The account that I have just given explains all the magnetic phenomena of the electric current observed up to the present time, and leaves you to foresee all those of which the conducting body in this state is capable. It is evident that the phenomena of ordinary magnetism differ from those of the current in this, that in the latter there is a double and inverse polarity while in ordinary magnets there is only simple polarity; and well as they can imitate artificially the double magnetic polarity we know of no means as yet of imitating by electricity the simple magnetic polarity. There is then a difference well established between these two states of magnetic polarization so that they cannot attribute the magnetic phenomena of ordinary magnets, that is to say, of simple polarity to an electric current which would continually traverse them in a direction perpendicular to their axis of polarization as M. Ampère has just conjectured. Some learned men pretend that our globe has four magnetic poles—but if it be thus the homologous poles are turned to the same side so that they may consider them as one, interrupted by some non-magnetic substance.

M. Ørsted has had recourse to an hypotheses really ingenious but very improbable, to know that the magnetic phenomena of the current owe their origin to a special movement of the two electricities of which the one determines the position of the needle above and the other below the wire conductor. The double magnetic polarity of the conductor appears to me an explanation much more simple and more probable.

SIR AND DEAR FRIEND.

I think you propose publishing in the "*ANNALES DE CHIMIE ET DE PHYSIQUE*" the Letter of M. Berzelius to M. Berthollet, which was communicated to the Academy in the Session of the 8th. January, 1821. The name alone of the author of this Letter would suffice to make all those who interest themselves in the progress of the Sciences, desire to know what this illustrious chemist thinks of the action between Voltaic Conductors, and Magnets discovered by M. Ørsted. M. Berzelius had written it before he could have had any knowledge of that, the existence of which I recognised in the month of September last, between the voltaic conductors, and of the consequences that I have drawn from them relative to the identity of electric fluid, and magnets: the experiments that he has described, follow immediately from the Theory founded on this identity, and I believe it useful to the progress of this branch of Physic, to give you some of the reflections the reading of it at the Academy, suggested to me, that you may also insert them in the "*ANNALES*" if you judge them worthy to appear there.

The observations of M. Berzelius on the cause of M. Ørsted in his experiments, having obtained only a deviation always less than a right angle, leave no doubt of what this cause is in the terrestrial globe, which combined in these experiments, with that of the voltaic conductor, and I will only speak of it here to remind you that, in the Session of the Academy, 25th. September, 1820, I had shown the same thing in a direct experiment, by the aid of an instrument represented in the IVth. Plate of the XVth. Volume of the "*ANNALES DE CHIMIE*:" the magnetic needle of this instrument being only able to move by turning in a plane perpendicular to the needle of inclination, the action of the terrestrial globe tended no longer to give it any determined direction; and it was proved in this Session, that when the voltaic conductor acted upon it, it placed itself constantly in a direction perpendicular to its own.

M. Berzelius examined the effects produced on the magnetic needle, by a very fine leaf of tin foil placed in the plane of the magnetic meridian, and of which the width was, in his experiments, two inches—when this foil communicates with the two extremities of the pile, it ought to form on it a series of electric currents, in the direction of its length, and following horizontal lines: of these currents those which are above the horizontal plane made by the point of suspension of the magnetic needle, submitted to the action of the tin foil, ought,

according to the Theory, to make the needle turn in one direction, while those that are below the same plane, ought to make it turn in the opposite; from whence it follows as M. Berzelius has observed, that the *maximum* action takes place, in the first case, when the needle is on a level with the lower edge of the tin foil; and in the second when it is on a level with the upper edge, since it is only then that all the currents act on the needle in the same direction at the least distance possible. When the needle is higher than the upper, or lower than the under edge, all these currents still act in the same direction, but the increase of the distance, decreases the intensity of their action on the needle—when it is at a height greater than that of the lower and less than that of the upper edge, part of the currents act in one and the remainder in the contrary direction; the effect is produced only by the difference of their actions; it is consequently less than in the case in which they all act in the same direction—when the needle answers to the middle of the width of the tin foil, this difference becomes nothing, and the action of the electric currents to turn the needle in the horizontal plane which passes through its centre of gravity being also the same. The needle returns by the terrestrial action in a plane parallel to the magnetic meridian, but if its extremities could then be raised or lowered in this plane it would become inclined on approaching a vertical situation, or would have its south pole to the left of the electric currents of the tin foil; it would even place itself in this vertical situation if it were not for its weight which hinders it, when the needle is suspended as it usually is, because its centre of gravity being below the point of suspension, the weight tends to bring the needle back to the horizontal direction. This kind of inclination produced by a voltaic conductor in a magnetic needle, which is parallel to it and situated in a similar horizontal plane, observed by M. CErsted, is the same fact as the deviation of this needle when it is placed above or below a horizontal conductor parallel to the magnetic meridian, as I have made it appear in my first memoir on these phenomena, when, from the 18th of September last I have re-united both these cases under this general enunciation.

“When a magnet and a conductor act on one another, one of them being fixed the other can only turn in a plane perpendicular to the shortest distance from the conductor and from the axis of the magnet, that which is moveable tends to move so that the directions of the conductor and the axis of the

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magnet make a right angle, and that the pole of the magnet which actually faces the North (the south pole) may be to the left of the electric current,\* it is easy to see that in reality, when the magnetic needle is above or below the conductor, the line which measures their shortest distance is vertical, so that, according to this general rule, the needle tends to turn in a horizontal plane: when the needle is at the side of the conductor, the line which measures its shortest distance is horizontal, and the needle, according to the same rule, tends to turn in a vertical plane."

"The two experiments that M. Berzelius relates afterwards in his Letter, come again in this, of which I have just spoken; but they are not less interesting to Philosophers, in proving that whatever may be the form of the outline of the metallic leaf, which forms part of the voltaic circuit, there are established in every point of this leaf electric currents, directed from the positive to the negative extremity of the pile. The currents ought, according to the Theory, to be disposed of in the first of these two experiments as they are seen in Fig. 12. and in the second as in Fig. 13: it is easy to conclude from this what would be the action they ought to exercise over the magnetic needle, and this action would be found to be precisely what M. Berzelius observed."

"The author insists on what, in this last experiment, in which the tin foil has the form of a square A.B.C.D. (fig 13) which communicates with the pile by the angles A.B. the *maximum* deviation of the magnetic needle takes place when it is in one of the two horizontal planes which pass through the angles C.D.; but it is evident that cannot be otherwise, whatever may be the cause of the phenomena, since it is only then that the electric currents are at the smallest distance possible either above or below the needle."

"M. Berzelius correctly remarks afterwards, that all these phenomena would have taken place if he had used, instead of the tin foil, a thin artificial magnet—wider than long: but that the foil presents yet one phenomena that the magnet would not have produced, viz: that the upper edge of the foil always makes the needle turn the same way, whether it be placed on the East or on the West of this edge in the horizontal plane in which it is found; whilst a thin magnet—wider than long, such as is represented in Fig. 14. supposing its poles in C. & D. under the same circumstances, turns the Magnetic Needle in one direction when it is placed on the East, and in the opposite direction when on the West.

\* "Annales de Chimie et de Physique," Vol. XV. p. 197.

This observation confirms better than anything what I have said concerning the disposition of the electric currents of magnets to which are due all their properties.

In reality, the tin foil A.B. (Fig. 15.) communicating in A. and B. with the two extremities of the pile, is a system of horizontal currents, which being all, in this experiment, lower than the Magnetic Needle, make it move always in the same direction, and as a single conductor placed under it would do: but the magnet A.B. (Fig. 14.) which M. Berzelius has supposed to be substituted for this tin foil, is an assembly of two systems of currents; the one formed by the currents on that side of the magnet which in the figure appears in front, and which go, for example, from A. to B., the other by the currents which return from B. to A.: on the other side of the magnet—that is to say behind it, when it is looked at as it is represented in the figure. When the Magnetic Needle is placed in an horizontal plane, passing through the upper edge E.F. of this magnet, whether to the East or West, it takes the direction that the nearest of these two systems of currents tends to give it; and as their directions are opposite, they tend to make it also turn in contrary directions; when the needle is on the East of the magnet, it is the nearest of these systems of currents which determines the direction of its motion; when it is on the West, it is nearest the other system, and consequently moves in an opposite direction.

We must not lose sight of the view which I have established on a great number of facts, that it is not on the poles of magnets, but on the electrical currents, which have place on the planes perpendicular to their axes, that the action of electric currents is exercised, whether it be of a voltaic conductor or a magnet, and that this action tends entirely to bring the first into the situation of the second, in such a manner that they are driven in the same direction as these last: we see by that, that a single system of currents as in that of the tin foil for example, ought always to make the magnetized needle turn to the same side, as we place near to it the horizontal plane, passing by its superior extremity, whether it be to the east or to the west of this extremity, and that it is necessary to have two systems of currents sent in a contrary direction on the two opposite sides of the magnet, of which M. Berzelius speaks, in order that this magnet makes the needle move in two contrary directions, according as it is to the east or west, as it is made to move in fact in the experiment described by this great philosopher.

He supposes, however, that it is in tin foil that is found, that which he calls a double polarization, and that polarization is simple in a magnet; he supports himself on this data of the experiment: if we place near the magnet, which he compares to the plate of tin, another magnet similar to the former in such a manner that they touch throughout their whole extent on one of their principal faces, such is the face G.H.I.K. (fig. 14), and that the poles of opposite species are found on the same side in the two magnets, in such a manner that one has lowermost, for example, its south pole and the other its north pole, this arrangement of the two magnets will act on the needle as the tin foil; but this is also a necessary consequence of the theory which I have exposed in the vol. 15, *des annales de chimie et de physique*, for he had then 4 systems of currents, two in each magnet. Those of these systems which are found in each magnet on the side of the face by which they are connected, are then in the same direction, and those which are found on the opposite side have place in the other direction, the one of these latter is always found nearer the needle than the former, it is their action which is always préponderant; and consequently, whether it is placed to the east or to the west of these two magnets, the needle which we always suppose to be in the horizontal plane passing by their superior extremity, it will take in the two cases the same direction, as when it is confined by the single system of electrical currents of tin foil.

M. Berzelius adds that though we may imitate with the voltaic pile, that which he calls double polarity, we cannot imitate that which takes place in magnets, and which he names simple polarity; this is the only objection which he opposes to the manner in which I have explained the mutual action of a magnet and a conductor, discovered by M. Ørsted, and that of two conductors which I first observed. But this perfect imitation of all the phenomena which a magnet presents is precisely the same that I obtained with the aid of an instrument described in the 45th Vol. *des annales de chimie et de Physique*, and represented in the plates thereto subjoined (pl. 2, fig. 3), by means of a metallic wire, which is put in communication with the two poles of the pile, and one part of which is enclosed in a glass tube, and the other is wound helically around the tube. After the mathematical laws of the action which the Voltaic Conductors exercise, that of the part wound as an helix, composes itself of two others, one of which is equal to that which a rectilinear conductor would produce placed in the axis of the helix, and of

the same length as this axis; and the other to that which would result from as many circular electrical currents, in planes perpendicular to this axis, as the helix has spirals. The primary action is destroyed by the equal and opposed action of the part of the conductor enclosed in the tube, in such a manner that there only remains that of the circular currents perfectly identical, as the proof of the experiment, to that of a magnet which we simply substitute for this tube; an identity which is verified either by suspending the tube as the needle of a compass, and submitting it to the action of a magnet, or of a wire-conductor, or by making use, on the contrary, of this tube as a magnetic bar, to study its action on the needle.

It is this experiment which shows, in a more direct manner, the identity of the Magnetic and Electric fluids; it responds at the same time to all the objections which have been made to me on this—that in supposing the magnet composed, as I have said, of Electric currents, there will not result therefrom all the phenomena which magnets present; but it is not solely on that that I establish this identity: all the facts which I have observed during six months, and your discoveries on the magnetizing of steel, by produced electricity, whether by a voltaic pile or by an ordinary electrical machine—both go to prove the truth of the theory which ranges all the phenomena of the magnet, amongst the phenomena purely electrical; and I doubt not that this theory would not very soon be generally adopted, if in place of limiting oneself to the examination of the mutual action of voltaic conductors on magnets, we were occupied with the same care on the action which two wire-conductors exercise one on the other, and of that of the terrestrial Globe on a conductor. In multiplying and varying the experiments on this subject, we observe at each instant, effects of which we cannot give the reason, except in departing from the theory which I have given of these phenomena, and which by running down so immediately, that it puts it in our power to make use of it—to foresee their advance. These experiments only exact for the rest a voltaic preparation of a mean force; I have made all mine with a pile formed of a dozen plates of zinc of a foot square, plunging them into the acid liquid indicated by M. Ørsted, in such a manner that the total surface of these plates, in contact with the acidulated water contained in the vessels of copper of a foot deep and a foot wide, on a breadth of eight or ten lines; was more than twenty feet square.

These observations which you have just read, M. and dear friend, have inspired me with the desire of indicating to the

savans who put the question of the identity of the electric and magnetic fluids, the importance which I consider this question possesses for the progress of philosophy; some of those motives which have induced me to regard this identity as demonstrated. Very far from having the idea of combatting the illustrious author of so many discoveries in the different branches of Science, Chemistry, and Physics,—it is to M. Berzelius himself that I submit the remarks, which were suggested to me by the reading of the Letter which he wrote to M. le compte Berthollet. I should be too happy if they do not appear to him unworthy of his suffrage.

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*Researches on the Action of the Alkaline Peroxides on the Metallic Oxides.* LETTER DE M. ED. FREMY to M. PELOUZE.

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[Read at the Academy of Sciences, in the Session of January 4th 1841.\*]

The evident analogy which exists between Iron and Manganese, leads us to suppose that we may one day produce with Iron, all the compounds which have been obtained with Manganese.

It is for this reason that I have thought it will be possible to form salts, in which a compound oxide of iron will act an electro-negative part, and which will thus correspond to the combinations of permanganic and manganic acids, with the bases.

The facts which I am about to make known, demonstrate, I believe, in a positive manner, that iron can give birth to bodies which produce themselves in the same circumstances as the manganates, and which present a striking analogy with this last class of salts.

When we heat during a length of time to the temperature of a bright red, a mixture of potash and of peroxide of iron, we obtain a brown mass, which re-taken up by water, gives a solution of a very beautiful red violet, and which possesses characters which I will indicate at a future time.

This compound may perhaps be more easily prepared, and in some minutes, in calcining, at a very high temperature, a mixture of nitre, of potash, and of peroxide of iron; or better still, a mixture of peroxide of potassium and of peroxide of iron.



I have succeeded in re-producing this body in the humid way, in making a current of chloride pass into very concentrated potash, holding in suspension the hydrate of the peroxide of iron.

This compound, obtained by these different processes, is of a beautiful violet; it is very soluble in water, a great quantity of water decomposes it at length: it becomes insoluble in very alkaline water, forming then a brown precipitate which is dissolved very well in pure water, and gives a solution of a fine purple colour. It appears much less stable than the manganate of potash. In certain circumstances it is decomposed at the ordinary temperature of peroxide of iron, which is precipitated in pure oxygen which disengages itself, and in potash which becomes free; the liquid is in this case completely discoloured. A temperature of  $100^{\circ}$  causes it to undergo a similar decomposition, but which is instantaneous.

All the organic matters decompose it, it is consequently impossible to filter the solution.

Such are the different facts which I have tested, and which tend to cause the admission of the existence of a body more oxygenated than the peroxide of iron. It has been with me hitherto, a matter of impossibility to insulate this compound; for when I have attempted to treat the red solution with an acid when the potash is saturated, it causes a disengagement of oxygen, and a precipitate of the peroxide of iron. If the acid is in excess it dissolves the peroxide, and forms a salt of the peroxide of iron.

In the Memoir which I shall have the honour of presenting to the Academy, I propose to give an analysis of these moveable compounds, which will be found based on the complete decomposition which they undergo under the influence of a temperature of  $100^{\circ}$ : and which permits us to measure the oxygen disengaged, and the peroxide of iron which is precipitated.

I shall discuss the different modes of investigating their composition, by examining if we can admit in these bodies the existence of alkaline peroxides. I shall make known also the results which I have obtained by placing the other metallic oxides in the same circumstances.

As the bodies which form the object of my researches are of delicate preparation, and as the study of them ought necessarily to be of a lengthened character, I thought it my duty to take a date;—to announce from to-day the primary results which I have obtained.

*On the presence of Iodine in the Oil of the Liver of the Cod Fish*, PAR W. STEIN. †

M. Stein concludes from his researaches on this subject, that we can only discover with certainty the existence of iodine in the oil of the liver of the cod fish by carbonization of soap, obtained with this oil. The following process seems to him to be the best: we treat at least 125 grammes of the oil to be examined by an excess of a solution of potassium equally distributed, at a temperature that will not reach to the ebullition of the mass, until the whole of the liquid is evaporated. We put the residue by portions [because the mass heats more easily] in a large crucible, and charcoal is placed with precaution on the crucible, and towards the end of the operation, a cover which is well adapted for the purpose, to hinder the volatilization of the combination of formed iodine. We then boil, renewing the operation several times, the carbonaceous residue with alcohol: we evaporate the liquors to dryness; we then dissolve the residue in a very little water; we sursaturate the solution by a small quantity of sulphuric acid, and then add either carbonate of sulphur, or what is as good a little recently-made solution of starch, with a drop of a solution of chloride of lime. In this manner would be discovered the least trace of iodine, and we may be assured that in fact the pure oil of the liver of the cod fish always contains a trace of it.

A. G. Y.

*Notice on Prussian Blue.* By WOHLER.\*

L. Gmelin has already made the remark that the Prussian Blue, which is obtained by precipitating the red ferro-cyanide of potassium by a protoxide of iron, ought to have another composition than the blue produced by the ferro-cyanuret of potassium and a *tritoxel*.

The first ought, in fact, only to contain for 3 atoms of cyanuret of iron, 1 atom of oxide of iron; whilst the ordinary Prussian Blue contains 2 atoms of cyanide. That which is here surprising is, that the two precipitates have the same blue

† Journal für praktische Chemie, Vol. XXI. chap. 5. p. 308.

\* Annalen der Chemie und Pharmacie, Vol. XXXV. chap. 3. p. 359.

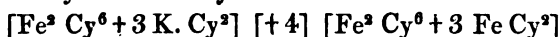
colour, and present besides the same properties. M. Vœlckel has at my invitation, annalized the Prussian Blue formed with the double cyanide. He has obtained it by pouring out by drops the double salt in a solution of chloride of iron, in such a manner that a portion of this latter remained undecomposed, and that the ferrocyanate of potassium was not added in excess. The precipitate was completely washed with cold water. It was of a strong blue drawing towards the capricious red.

The quantity of cyanogen was determined by the combustion with the oxide of copper, and that of iron by the calcination with the air, the solution in chlorhydric acid and the precipitation by ammonia.

In this case as in that of the combustion of this Prussian Blue, in oxygen gas, and subsequent reduction of the oxide in gas, we see that it contains a combination of potassium. The analysis has given the following proportions of its constituent principles, abstraction made by that of water, which has not been determined :

Iron .....	25,589
Potassium .....	5,284
Cyanogen .....	33,684

It seems to result from this analysis, that the blue precipitate obtained with the ferrocyanate of potassium is a combination of 1 atom of ferrocyanate of potassium, with 4 atoms of a Prussian Blue, which for 3 atoms of cyanuret of iron, contains only 1 atom of cyanide. A like combination—



will contain

	In 100 Parts.	
Iron .....	25,589	39,689
Potassium .....	5,032	7,805
Cyanogen .....	33,951	52,506

100,000

A. G. Y.

*Physical Optics. Application to Medical Questions.* By  
M. BIOT.\*

The work which forms the object of this note has been undertaken at the request of several persons who had enquired of M. Biot if the observation of rotatory power would not be sufficient to detect the presence and the proportion of sugar in the diabetic urine.

\* JOURNAL DE PHARMACIE.

The researches made by M. Thenard in 1806, by M. Chevreul in 1815 and by M. Bonuhardat in 1839, conduced to the idea that it was probable that the diabetic urine might contain two distinct sorts of sugar, the one sapid, the other insipid. But M. Bonuhardat has been assured quite recently that the non-sapid substance is a combination, or a mixture of sapid sugar with the lectate of uren, of chloride of sodium, and a small portion of extractive matter. The optical examination confirms this indication for the purification of the insipid product, by reiterated washings with cold alcohol-leaves, as a residue, a sapid sugar, analogous by its aspect to sugar of fecula, susceptible of the alcholic fermentation like it, and which exercises a rotatory power of the same kind. We know also cases of that malady, in which it produces in like manner an increased secretion of urine, but not susceptible of fermentation, and from which could not be obtained any more solid sugar. The absence of this substance is also as easily recognized as its presence by optical processess, and this pathological case will be thus immediately distinguished from the former. I shall now give a few words—the results of experiments made by M. Biot.

The urines secreted in the state of habitual health, do not present any indication of rotatory power. The solid sugar of diabetes, be it pure or combined, presents a rotatory power of the same species, and of the same order of intensity as that of ordinary sugar of amidon, that which agrees with the identity of powderable composition which the chemical analysis attributes to them. The diabetic urine presents a power of rotation directed towards the right of the observer, consequently in the same direction as the solid sugar of diabetes, which has been produced by evaporation; the energy of this deviation is proportional to the degree of dabetism.

M. Biot indicates the employment of optical characters as being capable of being used advantageously in many other cases. Thus, if the sugar of diabetic persons incorporates itself in some proportion with their blood, it becomes dissolved of itself in the water of the serum; then the proper power will be thereby enfeebled, or even overthrown, and it will suffice very well in small quantities, in order that the first or even the second of these effects becomes immediately manifest. We ought to observe a direction similar to the rotation towards the left, in the nephrites purely albuminous, when the urines will be transparent enough, or will have been sufficiently discoloured to permit light to be transmitted. "But already,

says M. Biot, "in limiting the application of optical characters to other cases, we see that they will furnish a sure diagnostic, exact, and of the most easy application to prove in a moment the diabetic state of urinary secretions. By that we shall be able to recognize the commencement of this malady from its first indications, to discern immediately the diverse peculiarities of it, to follow it in all its phases, to know instantaneously the effects, good or bad, of the regimen to which we submit those afflicted with it, as well as the specifics which we may wish to try on them."

M. Biot terminates by putting to the vote, that the medical men which are at the head of the Hospitals of Paris consent to establish, at least in one of the two, apparatus proper to make the application of optical characters, which he had just indicated.

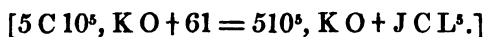
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*On the Action of Iodine on the Chlorate of Potash.* By  
M. E. MILLON.\*

There does not take place, in a cold state, any sensible action between the chlorate of potash and iodine; but if we pour on a determined quantity of chloride of potash, three or four times its weight of distilled water, raised to almost the temperature of the boiling point, the iodine which is added to this solution disappears in considerable quantity, whilst the liquor remains colourless. It remains uncoloured so much, that it does not go beyond in a very observable degree in the addition of iodine, the proportion of an equivalent by relation to the quantity of chlorate employed. When we have arrived at this stage, the liquid becomes of a sensible yellow colour, then a brown, and we obtain as a final result, the neutral iodate of potash and of chloride of iodine, more or less iodized. If we evaporate it to dryness, the chloride of iodine is disengaged, and the iodate of potash remains pure. In arresting the action of the iodine on the chlorate, before we have attained the proportion of an equivalent of iodine, we find that the liquor contains already the iodine formed, and more of chloride of iodine, which corresponds, without doubt, with the iodic acid; for if we heat it sufficiently strong, there is a disengagement of chloride, and there remains chloride of iodine I.C.L<sup>3</sup> which gives a precipitate of iodine by the carbonate of potash.

The formation of the chloride of iodine explains the re-action: the iodine solicits the chlorine of the chlorate to combine with

it, and to bring off the chlorate, whilst that the stronger affinity of the iodine for the oxygen, and the more considerable cohesion of the iodate, makes the iodine enter the chlorate in the place of chlorine. The re-action may thus be represented:—



## PROCEEDINGS OF THE ROYAL SOCIETY.

### SELECTIONS.\*

Feb 11.—A paper was read, entitled, “On a remarkable property of the Diamond.” By Sir David Brewster, K. H. D. C. L., F. R. S. L., V. P. R. S. Ed.

On re-examining the phenomena of parallel bands of light and shade exhibited by reflexion at the plane surface of a diamond, which the author had noticed some years ago, he concludes that they result from the reflexions of the edges of veins or laminæ, of which the visible terminations are inclined at different angles, not exceeding two or three seconds, to the general surface. He gives an account of several analogous facts observable in other crystals, more especially those of carbonate of lime, artificially polished in surfaces inclined to the natural planes of cleavage.

March 11.—The following papers were read:

1. “On a Cycle of Eighteen Years in the Mean Annual Height of the Barometer in the Climate of London; and on a Constant Variation of the Barometrical Mean, according to the Moon’s Declination.” By Luke Howard, Esq. F.R.S.

For obtaining the general results communicated in the present paper, the author has followed the same method as that he had adopted in the two former papers laid before the Society on the connexion of the barometrical variation with the lunar phases and apsides. Tables are given of the barometrical averages on the successive solar years, from 1815 to 1832, so constructed as to exhibit the variation of the moon’s influence according to her declination; and also of these averages on successive cycles of nine solar years, classed according to the moon’s place in declination, on either side of the equator. The results deduced from these comparisons are, first, that the barometrical mean in this climate is depressed by the moon’s declination being to the south of the

\* Continued from Page 254.

equator; and, secondly, that this depression takes place gradually, commencing with the moon's being in full north declination, and proceeding through her remaining positions to the time when she crosses the equator to resume the northern declination; at which season, the whole pressure that had been withdrawn from the atmosphere is suddenly restored. The author thinks there is evidence of a great tidal wave, or swell in the atmosphere, caused by the moon's attraction, preceding her in her approach to, and following her slowly as she recedes from these latitudes; so that were the atmosphere a calm fluid ocean of air, of uniform temperature, this tide would be manifested with as great regularity as those of the ocean of waters. But the currents uniformly kept up by the sun's varying influence effectually prevent this from taking place, and involve the problem in complexity. He finds that there is also manifested in the lunar influence a gradation of effect, which operates through a cycle of eighteen years. The mean pressure of the atmosphere during the first part of this period increases; and then, after preserving for a year its maximum amount, again decreases through the remaining years of the cycle, but exhibits, towards its minimum, some fluctuations before it again regularly increases.

2. "On a remarkable depression of the Barometer in November 1840, agreeing very closely in its movements and results with that of December 1821." By Luke Howard, Esq., F.R.S.

The object of the author in the present paper is to show the close correspondence of the extraordinary depression of the barometer in the months of October and November of last year (1840), and of the remarkably stormy weather which prevailed at the same period, with similar phenomena occurring in December 1821, when the moon's place in declination underwent the same changes during those two periods, at an interval of nineteen years.

"Note on an irregularity in the Height of the Barometer, of which the argument is the Declination of the Moon." By Sir John William Lubbock, Bart. V.P. and Treas. R.S.

In the Companion to the British Almanac for 1839, the author inserted some results, which were obtained with a view of ascertaining the influence of the Moon on the barometer and on the dew-point. Mr. Luke Howard's researches on this subject having recalled his attention to that paper, he found that some of the results he had given appeared to indicate that the Moon's position in declination influences

the barometer. In order to render this more manifest, he combines in the present paper all the observations he gave in the Companion to the British Almanac in three categories. These observations correspond to different angular distances of the moon from the sun, or times of transit: but as the inequality of the ocean, of which the argument is the moon's declination, is independent, or very nearly so, of the time of the moon's transit; it is probable that so also is that in the height of the barometer. In this case we may, with propriety, combine in the same category observations which correspond to similar declinations, although to different times of transit. The results stated by the author seem to indicate an elevation of nearly one-tenth of an inch for 17 degrees of declination. The inequality has a contrary sign to the inequality of the same argument in the tides of the ocean.

April 1. The following Letter, addressed to the President was read:—

“4, Trafalgar Square, London, March 25th. 1841.

MY LORD.—I have the honour of transmitting to Your Lordship, for presentation to the Royal Society, an original portrait of SIR ISAAC NEWTON, by *Vanderbank*, a Dutch painter of some note in that age.

“This picture has now been many years in my possession, and the tenure by which I have kept it [as a collateral descendant of so illustrious a man] was too flattering not to have been a source of great personal gratification.

“But I consider this portrait to belong, of right, to the Scientific world in general, and more especially to that eminently distinguished Society of which Newton was once the head, and which is now so ably presided over by Your Lordship.

“I have, therefore, to request Your Lordship will do me the honour to present this original portrait of Sir Isaac Newton to the Royal Society in my humble name.

“Accident having destroyed some of the papers of my family, I am unable of myself to trace the entire history of this portrait, but I believe more than one member of the Royal Society is competent to do so, and it is well known to collectors: and a small mezzotinto engraving of it was published about forty years ago. It was painted the year before Newton died, and came into the family of the celebrated Lord Stanhope, who left it by his will to my grandfather, the late Dr. Charles Hutton, a distinguished member of the Royal Society, expressly on the well-authenticated account of that eminent mathematician, having been remotely descended from Sir



Isaac Newton, in the following way, as I find on a family manuscript; viz.—“that the mother of the well-known James Hutton and the mother of Dr. Charles Hutton were sisters; and the grandmother of James Hutton and the mother of Sir Isaac Newton were also sisters.”

“I have ever considered this very distant connexion with so great a man should not be an inducement to lead me into any but casual mention of the circumstance, that I might avoid the imputation of a vain boast; nor would it have been brought forward now, except to explain the cause by which this portrait came into the possession of an individual who is happy in relinquishing it to grace the Hall of Meeting of the Royal Society.

I have the honour to subscribe myself,  
Your Lordship's very obedient humble Servant,

CHARLES VIGNOLLES.”

“*The Right Honorable the Marquess of Northampton,*  
    &c. &c. &c.  
*President of the Royal Society.*”

A paper was also in part read, entitled, “On the proportion of the prevailing Winds, the mean Temperature, and depth of Rain in the climate of London, computed through a cycle of eighteen years, or periods of the Moon's Declination.” By Luke Howard, Esq., F.R.S.

April 29.—The reading of Mr. Howard's paper was resumed and concluded.

In this paper the author investigates the periodical variations of the winds, rain and temperature, corresponding to the conditions of the moon's declination, in a manner similar to that he has already followed in the *barometrical variations*, on a period of years extending from 1815 to 1832 inclusive. In each case he gives tables of the average quantities for each week, at the middle of which the moon is in the equator, or else has either attained its maximum north or south declination. He thus finds that a north-east wind is most promoted by the constant solar influence which causes it, when the moon is about the equator, going from north to south; that a south-east wind, in like manner, prevails most when the moon is proceeding to acquire a southern declination; that winds from the south and west blow more when the moon is in her mean degrees of declination, going either way, than

with a full north or south declination, going either way, than with a full north and south declination; and that a north-west wind, the common summer and fair weather wind of the climate, effects, in like manner, the mean declination, in either direction, in preference to the north or south, and most when the moon is coming north.

He finds the average annual depth of rain, falling in the neighbourhood of London, is 25·17 inches.

From his observations on the temperature, he deduces the following conclusions:—1. That the pressure of an atmospheric tide, which attends the approach of the moon to these latitudes, raises the mean temperature 0·35 of a degree. 2. That the rarefaction under the moon in north declination lowers the temperature 0·13 of a degree. 3. That the northerly swell following the moon as she recedes to the south further cools the air 0·18 of a degree. 4. That this cold continues while the moon is away south, reducing the mean temperature yet lower by 0·04 of a degree.

May 27.—The following papers were then read, viz:—

1. "On the Compensations of Polarized Light, with the description of a Polarimeter for Measuring Degrees of Polarization." By Sir David Brewster, K.H., D.C.L., F.R.S., and V.P.R.S. Ed.

In four papers published in the Philosophical Transactions for 1830, the author maintained, in opposition to the prevailing theory, that light, either reflected or refracted at angles different from that at which it is completely polarized, does not consist of two portions, one completely polarized, and the other completely unpolarized, but that every portion of it has the same physical property, having approximated in an equal degree to the state of complete polarization. This conclusion, which had been derived from reasoning on the hypothesis that a pencil of light, composed of two pencils polarized respectively at angles of  $+$  and  $- 45^\circ$  with the plane of reflexion, was equivalent to a pencil of common light, is confirmed in this paper by experiment, made with common light itself, reflected from different parts of the atmosphere, and from which the uniaxal or biaxal systems of rings were obtained. On placing such a system between light partially polarized in an opposite plane, the author found that the rings disappeared, the direct system being seen on one side of the plane of disappearance, and the complementary system on the other side. In the experiment the polarization of the light in one plane was compensated by

the polarization of the same light in the opposite plane; and, consequently, both the pencils, which had undergone the two successive polarizing actions, had received the same degree of polarization in opposite planes; and in virtue of these two equal and opposite polarizations, the light at the point of compensation, where the system of rings disappeared, had been restored from partially polarized to common light; and the light on each side of this point of compensation was in opposite states of partial polarization.

In order to give a distinct view of the nature of this experiment, the author details the phenomena observed at particular angles of incidence on glass. From the results at an angle of incidence of  $24^\circ$ , the ray suffering one refraction at  $83\frac{1}{2}^\circ$ , he concludes that the compensation which takes place is produced neither by an equality of oppositely polarized rays, nor by a proportional admixture of common light, but by equal and opposite physical states of the whole pencil, whether reflected or refracted.

The remarkable phenomena produced at an angle of incidence on glass of  $82^\circ 44'$  [at which angle  $\cos [i + i'] = \cos^2 [i + i']$ ], led the author to the construction of what he terms the *compensating rhomb*, consisting of a well-annealed rhomb of glass, or any other uncrystallized substance, having the angles of its base  $130^\circ 25'$  and  $46^\circ 35'$  respectively, when the index of refraction is 1.525. When a ray of light is incident upon the first surface at an angle of  $82^\circ 44'$ , exactly one-half of it is reflected; and the other half, after refraction, is reflected at the second surface, and emerges perpendicularly to the adjacent surface, without suffering refraction; each portion having, in the first instance, the same quantity of polarized light. The second portion is found, on examination, to be in the state of common light, although the ray at the second incidence consisted of more than one-half of polarized light. Hence if the pencil, previously to reflexion at the second surface, consist of 145 rays of polarized light, and 138 of common light, the effect of a single reflexion must be to depolarize polarized light, and to produce no change whatever upon common light; a property of a reflecting surface never yet recognized, and incompatible with all our present knowledge on the subject of the polarization of light.

The author then describes an instrument which he has invented for the purpose of accurately measuring the degrees of polarization, and which he therefore terms a *Polarimeter*. It consists of two parts; one of which is intended to produce a ray

of compensation having a physical character susceptible of numerical expression, and the other to produce polarized bands, or rectilineal isochromatic lines, the extinction of which indicates that the compensation is effected. The construction and mode of operation of this instrument are, by the aid of figures, described and explained.

The following is the general law established by these researches; namely, that the compensation of polarized light are produced by equal and opposite rotations of the planes of polarization. Thus, when a ray of common light is incident, at any angle, upon the polished surface of a transparent body, the whole of the reflected pencil suffers a physical change, bringing it more or less into a state of complete polarization, in virtue of which change its planes of polarization are more or less turned into the plane of reflexion; while the whole of the refracted pencil has suffered a similar, but opposite change, in virtue of which its planes of polarization are turned more or less into a plane perpendicular to the plane of reflexion.

The author then enters into a theoretical investigation of the subject, and concludes by pointing out a few of the numerous application of this theory.

*W. Henley, on an Electro Magnetic Machine.*

Sir,

I take the liberty to introduce to the numerous readers of the *Annals of Electricity*, &c. an *Electro Magnetic Machine*, differing materially from the common description of that Instrument, viz:—The method of breaking battery contact, &c. Fig. 2, plate vii. represents the Machine consisting of primary and secondary coil A. and B. a cross of soft Iron, not covered, which rotates between two pieces of Iron C. C. passing through the sides of the bobbin A. so as to be in contact with the iron wires in the axis. D and E are two copper wires carrying the current which press upon the break-piece F. into which there is a piece of Ivory inserted for breaking contact. Upon making connection with the battery, the cross B. commences rotating rapidly, and, on placing a finger on each of the conductors F. F., a succession of shocks are felt, more violent than from any machine that I have as yet constructed.

When the toothed wheel and spring are used, the stud G.

cuts off the current from the wires D. and E. on moving the stud H. the conductors are disconnected from the secondary, and no shock is felt. F. is a platform, with two binding screws for connecting a voltameter, platina, wire, &c &c.

Fig. 2. is a more portable and less complicated modification of the same instrument.

Fig. 3. is a soft iron horse-shoe, covered with primary and secondary coils with a vertical iron cross revolving between the poles.

In this instrument the current passes along the brass piece D. on which B. revolves to the break-piece, from thence by the wire to the magnet.

Should you think the enclosed worthy of a place in your Annals, the insertion of the same will greatly oblige,

Sir, Your most obedient and very humble servant,

WILLIAM HENLEY,

Philosophical Instrument Maker,  
25, Back Church Lane, Commercial Road, East,  
19th. November, 1839.

To W. Sturgeon, Esq.

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*GALVANOPLASTIK; or the process of cohering copper into plates, or other given forms, by means of galvanic action on copper solutions. By Dr. M. Jacobi, Privy Councilor to the Emperor of Russia, and Member of the Royal Academy of Sciences, of St. Petersburg.\**

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## PREFACE.

The pages which I now submit to my readers contain such of my discoveries of the new application of the galvanic powers, as appear to be important with reference to practical and scientific individuals: and which, in some measure, already has become so. It happened, whilst I was in Dorpart, in the month of February, 1837, prosecuting my galvanic investigations, that I discovered a striking phenomenon which presented itself in my experiments, and furnished me with perfectly novel views. By an attentive observation and persevering pursuit of this phenomena, I soon became convinced that in this simple fact there lay a completely new field of interest; which by means of galvanic currents we might be

\* Translated from the German Edition.

able to arrive at successfully; but it was only by very gradual steps that I attained a knowledge of the simple conditions, on which the results are depending.

Towards the latter end of the same year, in consequence of the convocation at St. Petersburg, of their Excellencies the Ministers, for the people's moral improvement, in which I was entrusted with the command of the most important labours, I was considerably interrupted in this pursuit: notwithstanding however, I found intervals of time for occasional observations and experiments,, which I could afterwards turn to advantage. After the termination of this interruption, the results of my proceedings appeared with more certainty; and having succeeded in removing many casualties from the process, I made communication of my discovery, accompanied by a well-developed Galvanic production, to the Royal Academy of Sciences, which was read in the Session of the 5th. October, 1838, and first published in the "BULLETIN SCIENTIFIC," No. 95. An abstract of the same paper afterwards appeared in the St. Petersburg "GERMAN NEWS," of October 30th. by means of which it obtained much greater publicity.

His Excellency the Minister for moral improvement and President of the Academy, who had previously introduced it to that learned assembly, had also the kindness to place this *first* galvanic production before his Majesty our lord and King; and it was ordered that a second printed notice of my process should appear in the St. Petersburg "RUSSIAN NEWS," of the 24th. December. On the 29th. of the same month a translation of that article appeared in the "GERMAN NEWS": hence, even so early as this date, the information of my discovery had spread rapidly through the medium of different languages. It also appeared, about the same time, in many foreign languages; as for instance, in English newspapers, and scientific and other Journals.\* I purposely allude to these printed official documents, to prevent those, who, otherwise, from a knowledge of my discoveries, might be induced to proceed to experiments, and venture to refer to claims, by appearing to be totally ignorant of that previously done. The galvanoplastik process belongs to Russia exclusively. It was here that it originated, and here were its improvements acquired. The magnanimity of our Government, which always provides for the dissemination of useful knowledge, has given general publicity to this discovery in the most complete manner pos-

\* The first information we had of Professor Jacobi's discovery was from our Correspondent at Munich, in a letter dated February 5th 1839. See "Annals of Electricity, &c." Vol. 3, P.P. 507, 508. EDITOR.

sible: and at the same time has bestowed a valuable present to every other part of Europe.

Galvanism is a mighty multilateral operating agent, which was discovered about the commencement of the present century; and now, above all other subjects, engages the attention of philosophers in every part of the world. Until now, this power was known only in the quiet study of the learned, and in the laboratories of the philosopher and the chemist; and now for the first time, has it been made to advance and spread through an extensive circle of the Arts, and proceed to the hands of the Artificers and Manufacturers. You, my readers, will become acquainted with a power whose phenomena may not, perhaps, be strange to you generally, but whose laws and management may appear to some of you too mysterious, or at least too complex to be overcome. On this account I have thought it necessary to prepare, beforehand, a generally comprehensive representation of these laws: and in order to understand it, you must favour me by laying aside all former prejudice, so that you may be sufficiently informed in the theory to enable you to overcome all accidents and difficulties that may present themselves, which, though tedious, and almost impossible to enumerate, may be removed by attention and activity.

I have already had experience that men entirely without scientific cultivation, and without education, accomplished the galvanoplastik process, from the directions that I have imparted to them respecting the usual routine of manipulation of the galvanic apparatus. When indeed we consider that, at this day almost every one is acquainted with the use of the lever, the screw and the other mechanical powers:—that every body is acquainted with the pressure of air and water, and know how to explain the phenomena:—that the advantages of fire and heat, and of the elasticity of steam, have become generally understood:—that the complicated operations of chemistry in the manifold industrious movements in the arts and manufactures, are carried on with facility and exactness:—and if we look at all these powers and apparatus as they appeared at the commencement, and consider that they went out of the lap of the scientific philosopher, no less complex and mysterious than the galvanic appears to be now, we immediately arrive at the conviction that the day is not far distant, indeed is already at hand, when even this power amongst the rest will be cultivated to advantage by the industrious of all nations, and, like them, will henceforward

yield profits in its turn. Hence it is that, as the other scientific enquiries have become established and promoted:—as favourable operations and enquiries proceed unobstructed:—as the mistrusts and jealousies, by which the practical man and the man of science are kept at a distance from each other, disappear:—and when every one ceases to refuse the other the full acknowledgement of his right of discovery, then will Galvanism present a new example that even the smallest grain of seed scattered by the philosopher, will, sooner or later, produce its full share of fruit.

In the publication of this Essay I cannot conceal that I have laboured under a peculiar anxiety: for, I am well aware that the obstructions wherewith we are continually assailed in our enquiries become, eventually, so familiar that we hold them lightly and as mere trifles, and even entirely lose sight of the numberless troubles that attend experimental pursuits which led to the results; whilst a detail of those events would be extremely useful to the beginner. It is, however, my particular wish to place this Essay before you as an easy and familiar whole: to obviate the process, to prevent error and to make the pursuit familiar; and, as the saying is, not to leave you under the necessity of making the entire school for yourselves. I have, on this account, taken some pains in choosing and deciding on principles; and I have also described the management of them as distinctly as possibly I can. Nevertheless, I have been unable to arrive at that degree of perfection that I could have wished, so that I must request you to apply to me personally, in order that I may show you the apparatus and explain them more clearly: make you familiar with many manipulations which could not, without prolixity, be exhibited in a written form, and assist you generally with council and deed, whereby you may enabled, without embarrassment, to practice this new art and derive sure profits from your industry. I shall, hereafter, give public notice in the journals, on what days I may be consulted either at my own residence, or at some other place: where you will be received without distinction, and assisted in overcoming such difficulties as you may happen to meet with in this new sources of employment.

It gives me great pleasure to have this opportunity of publicly acknowledging the favours and assistance which I have received from those illustrious Statesmen, whose magnanimity is ever ready to promote the interests of Science, and every branch of useful knowledge.

St. Petersburg, March, 1840.

M. H. JACOBI.



## ON THE GALVANOPLASTIK

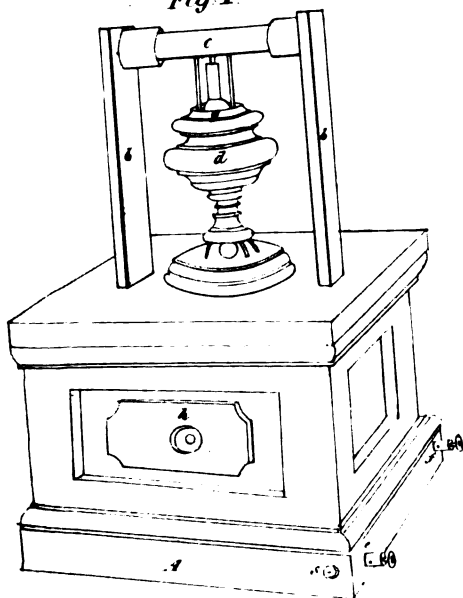
If a common zinc plate be placed in a vessel containing diluted sulphuric, or muriatic acid, it will dissolve more or less rapidly, and the developement of hydrogen, which in this case is not pure, will be known by the unpleasant stinking smell. If the zinc plate be withdrawn from the liquid and some drops of quicksilver be poured on it, the liquid metal will spread all over it; or, as we say in technical language, it will amalgamate; and the plate attains a silvery brightness. If we now again immerse the plate in the acid liquor, it will remain perfectly neutral, and unacted on by it, and may be kept there for some considerable time without suffering loss of weight. If we immerse a plate of copper in the same vessel with the amalgamated zinc plate, but without touching it, the condition of the latter is in nowise altered: but if we bring the two plates into contact with each other, either below or above the surface of the liquid; or even connect them by a metallic conductor, such as a wire or ribband of copper, brass, lead, or any other metal, we immediately perceive hydrogen in bubbles developed on the copper plate: but the zinc plate when well amalgamated, has no perceivable developement of gas; nevertheless it gradually corrodes; and by and by becomes entirely dissolved. This, and similar phenomena, which only occur between two different metals, which for example, I have chosen copper and zinc, when brought together in presence of a fluid, becomes indicative of a certain peculiar force, which we call *galvanism*.

We have already seen that a common zinc plate, independently of the operation of the copper, becomes dissolved in the acid solution; and we know that that fact indicates a chemical process. The water with which the acid is diluted, by the action of the zinc, becomes resolved into its constituent elements, the hydrogen and oxygen gases: the latter combines with the zinc and forms a compound which combines with the sulphuric acid, and sulphate of zinc becomes formed. The hydrogen is perfectly free and escapes in the shape of gas, similar decomposition also takes place by the galvanic influence, but with this difference in the nature of the action. The oxygen as before combines with the zinc, but the hydrogen appears on the surface of the copper plate, and finding nothing to combine with, makes its escape.

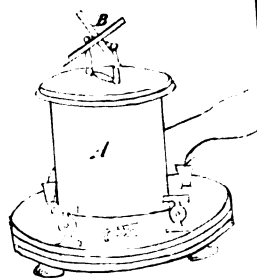
The apparatus represented by fig. I, plate 8, is usually called a *simple galvanic circle*; where C. is a plate of copper, Z. a plate of zinc, C.V.Z. a wire or strip of metal which connects the two plates together, and a.b.c.d. a vessel in which they are immersed in the dilute acid. We will just here remark, that the purpose of such a piece of apparatus is occasionally though but seldom, employed for obtaining a solution of zinc: which, as we have already seen, can be obtained by a simpler means. But we shall only notice those operations which to us are of greater importance, and which are due to the connecting wire, or other metallic piece which is used in place of it. Experience has shown that the strength of the action is somewhat proportional to the quantity of hydrogen liberated at the copper plate: but the bubbles of hydrogen which rises from the zinc not only nothing strengthens the action, but, on the contrary, absolutely weakens it. If we employ common, instead of amalgamated zinc, in a galvanic circle, there will be a compound, or mixed action; the chemical and the galvanic, which conjointly dissolve the zinc. The violence of the effervescence, and the rapid dissipation of the zinc plate, are not, in such cases, any standard of the galvanic power, which is indicated only by the quantity of zinc dissolved by virtue of the combination of the copper with the zinc plate. The dissipation of the zinc, which is brought on by itself alone may be considered as a useless loss; at least in reference to the galvanic activity which we wish to produce. It follows, we hold, from these considerations, that the galvanic power may be measured by collecting the hydrogen in a glass tube, whose capacity is already ascertained. Or we may arrive at the same thing by ascertaining the loss, by weight, which the zinc suffers in a given time. These measures, however, are somewhat uncertain: because after the zinc is even amalgamated, accidental circumstances occur by which small quantities of zinc become dissolved in the acid solution, which have no reference to the galvanic action. If, instead of the acid, we take solutions of salts, such as common salt, sal ammoniac, glauber salts, &c. the zinc itself, though not amalgamated, does not become corroded: and it is only when in galvanic combination with copper that it suffers any loss: and the loss which it then suffers has a close relation to the quantity of hydrogen liberated at the copper plate.

(To be Continued in the next Number.)

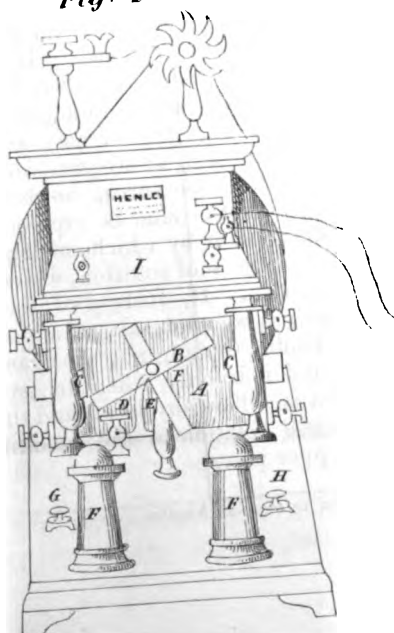
*Fig. 1*



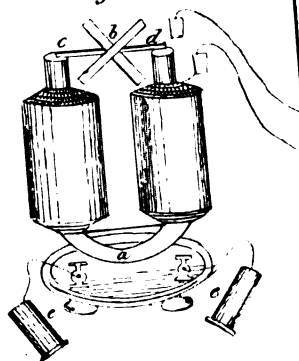
*Fig. 3*



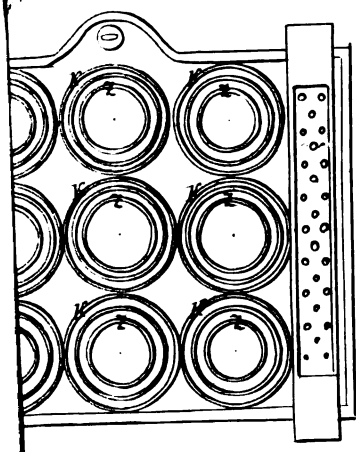
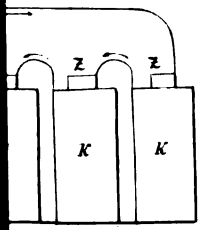
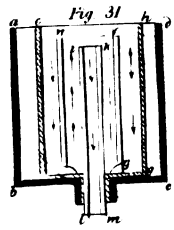
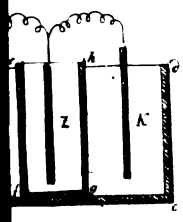
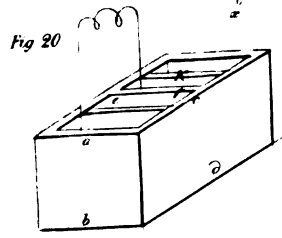
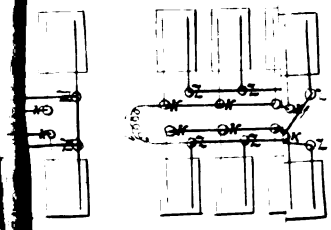
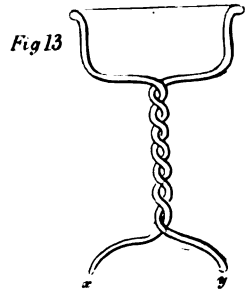
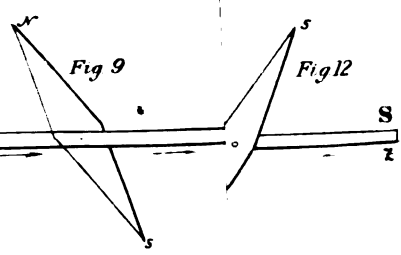
*Fig. 2*



*Fig. 4*







for the pointed wire connected with the inner surface gives such great facilities for the escape of the electric fluid, that a jar cannot

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100

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## ELEMENTARY LECTURES ON ELECTRICITY, &amp;c.

## LECTURE XII.

There is such a variety of experiments which might be brought forward by the employment of the Leyden jar, most of which are highly favourable to the Franklinian theory, that there is not so much difficulty in finding illustrations, as in knowing where to discontinue them in proper time; but as we are yet within bounds, I will offer a few more to your notice.

I will uncover the ball of the Leyden jar, and expose the pointed extremity of the wire which is connected with the metallic lining to the prime conductor, by bringing them into contact with one another. Now, as the machine is kept in action, the jar will charge in the usual way; and now that it is charged, I place it on a sheet of tin foil on the table. I now touch the tin foil with one hand, and with the back part of the other hand I approach the point of the projecting wire; and I experience an agreeable aura, or electric breeze, gently brushing against my hand. It now slackens its intensity, which, indeed, seems to diminish very rapidly. It has now ceased altogether, so that I may venture to touch the wire with my hand without danger of receiving a shock. Now in this experiment the jar has been discharged silently, and without any appearance of light, which were consequences of the discharge being accomplished gradually, through the medium of the points of the wire, and keeping the hand at a suitable distance: for had I brought my hand close to the wire suddenly, whilst the jar was fully charged, I should have received a violent blow in my arms and shoulders, as decidedly as if the wire had been surmounted with a ball.

This is a very instructive experiment, and may be varied in several different ways. If, for instance, when the jar is charged, I employ the discharging rod, instead of my hand, towards the point of the vertical wire, the discharge will be accomplished silently and without light, as decidedly as by the application of the hand; but in both cases, when either the hand or the ball of the discharging rod is brought sufficiently near to the point of the jar's wire, a singular crackling noise will be produced, and if the room be darkened, a thin crooked stream of exceedingly minute sparks will be observed. If the hand be applied for this purpose, a singular disagreeable prickling sensation is experienced on the skin; but the crackling noise is best produced by the ball of the discharging rod.

Another mode of varying the experiment is, that of first charging the jar, and then placing it on the table without any further manipulation. In the course of a few minutes afterwards the jar will have disposed of the whole of the charge into the surrounding air: for the pointed wire connected with the inner surface gives such great facilities for the escape of the electric fluid, that a jar cannot

possibly retain its charge many minutes under such circumstances. If the jar, when charged, be placed in a darkened room, you may have ocular evidence of the discharging process, by observing a beautiful brush of attenuated purple electric light issuing from the point of the wire into the atmosphere: and not only that fact will be observed, but another, of still greater interest. The electric brush, which first appears, whilst the jar is highly charged, is much larger, and by far more elegant, than during any other period of the exhibition; the reason of which is, that as the discharging process itself is occasioned by the repulsive forces amongst the congregated particles of the fluid on the inner surface of the jar, and as those particles are more numerous, and, consequently, their combined forces more powerful, at first, than at any other time, they then spring with the greatest degree of energy through the point into the air; and in proportion as the discharge goes on, the electric forces within the jar abate, and eventually become too feeble to drive off any more of the electric matter.

This property of metallic points of throwing the electric fluid into the surrounding medium, is sometimes taken advantage of in the charging process; and is, indeed, another mode of illustrating this property, as well as being also a peculiar mode of illustrating a certain part of the theory of the jar. Let us hang the jar on the wire of the prime conductor of the machine, as in fig. 1. plate vi: but instead of the second jar applied to the coating of the first, we will furnish that coating with a point. If now the room be darkened, and the machine be put to work, you will see an electric pencil of light going from the pointed wire attached to the coating of the jar. Now as this is the case, and as a jar charges when the fluid naturally belonging to its outer surface can make its escape, it is plain that the jar ought now to be charged; so that by applying the discharging rod in the usual way, we shall find that such is the case; for a bright flash of light and a smart report is immediately produced.

As it is not my intention, in these elementary lectures, to advance facts in direct opposition to the statements of other writers on this subject, for the mere purpose of refutation, I shall pass such statements unnoticed, and proceed as though they had never been made.

I will first shew you an experiment with the jar placed at some distance from the prime conductor, directly below it, and with the pointed wire directed upwards. When the machine is put into motion, the jar begins to receive a charge as decidedly as if the point had been placed in close contact with the prime conductor; and when the machine has been in action for a few seconds, we shall find the charge almost as high, if not quite so, as under any other circumstances. An application of the discharging rod immediately indicates this fact. Again, we will now place the jar on a support, so that the point shall be on a level with the axis of the prime conductor, and at the distance of two feet from it: the charging process will



go on as in the former instance, but not quite so rapidly; yet the charge thus attained will be tolerably high.

The question now is, did the jar in both these cases, or in either of them, receive the charge from the prime conductor? Let us try to decide this point by another experiment. For this purpose we will employ another neat little apparatus, represented by fig. 5, plate vi., and called an electrometer. It was invented by Mr. Henly, about the year 1770, and as the original description of it is given at the end of this lecture, I shall not say anything more about it in this place, further than that it is applied to the prime conductor by being stuck in a hole made for its reception on the upper side. The pith ball, *b*, terminating the index, is repelled from the main stem as the charge of a jar advances, and will sometimes attain a deflection of more than  $90^\circ$ , or, in other words, the ball *b* will rise to above the height of the hinge, or centre of motion. And, on the other hand, should the prime conductor, to which the instrument is attached, suffer any loss of fluid whilst charged, the decline of the index deflection immediately shows it.

Having now made you acquainted with the use of the electrometer, we will proceed to experiment, by first fixing the instrument in the prime conductor, and placing the jar at a distance beneath it; but we had better satisfy ourselves, in the first place, that the machine is in good action, and that the index of the electrometer stands high when the jar is not present. Having now found the machine in good action, by the index of the electrometer forming an angle with the lower part of the stem, of more than  $90^\circ$ , I will place the jar beneath the conductor, with its pointed wire at a distance of more than twelve inches, still keeping the machine in motion. Now let me solicit your attention to the electrometer, whose index has fallen full  $50^\circ$ , indicating a decline in the electric intensity of the primal conductor. I will now remove the jar for a moment, and you see that the index of the electrometer mounts up again as high as at first, which is a sufficient evidence to shew that no decline of energy in the machine has taken place; and therefore we may rest satisfied that, whilst the jar was present, the decline of the index of the electrometer did not arise from any corresponding decline in the action of the machine; and, therefore, the loss of intensity in the prime conductor was due to some other cause. Now we have already seen, by the previous experiment, that the jar can be charged by being placed in the same situation as in this; and as the only difference in the method of performing these experiments consists in the additional electrometer in the present case, we have only to try if the jar can be charged whilst that instrument is attached to the prime conductor. A few turns of the machine, and a proper application of the discharging rod, satisfy us that the jar can be charged as decidedly by the presence of the electrometer as without it. Now, from all these circumstances we have no other *legitimate* inference than that the jar received its

charge from the machine through the medium of the prime conductor.

If, instead of one pointed wire from the lining of the jar, we were to expose several to the electric atmosphere of the prime conductor, the index of the electrometer would fall much lower, and the jar would charge proportionally faster; and the same fact would be exhibited if, instead of pointed wires, we were to place a sod of grass on the top of the jar. The blades of grass draw off an immense quantity of the electric matter, and must necessarily be instrumental in collecting it in abundance from the atmosphere and in transferring it to the ground. Mr. Pine, of Maidstone, has made several interesting experiments with the leaves of plants.

Now, as we have seen by previous experiments, that sharp pointed wires, and, indeed, sharp pointed and sharp edge leaves of plants, grass, &c., are capable of throwing off the electric fluid with such great facility, we can easily take advantage of this fact, or, perhaps, rather illustrate it by another experiment. We will now take away the ball from the wire in the end of the prime conductor, and place a Leyden jar at a foot distance from it, either on the table or elevated to the same height. In this case, the point throws off the fluid into the atmosphere, and, to a certain extent, electrizes all the neighbouring bodies, amongst which is the Leyden jar, as may be ascertained by the usual method of applying the discharging rod. A point stuck into the end of the prime conductor will throw off all the electric fluid which can be excited by the most powerful machine; and a gold leaf electroscope, at the distance of twenty yards, may be electrified by that means.

Another mode of discharging the jar in a pleasing manner, is by means of a gilt pith ball attached to one extremity of a thin silken thread, and hung by the other extremity, so as the ball may be on the same level as the ball of the jar. Another brass ball, on the same level as the other two, is to be supported on a stout wire which is in connection with the outside of the jar. The two brass balls may be about three inches apart, and the gilt pith ball half way between them. In this case the whole of the balls will be electro-polar in the following manner. The ball of the jar will be electro-positive, and that in connection with the outer surface will be electro-negative, and the suspended pith ball will be both positive and negative: positive on that side next the negative ball of the jar, and negative on that side next the positive ball of the jar. But this condition of the arrangement will hold good only for a moment: the gravity of the light pith ball suspended between the two forces will be overpowered by one of them, and will be made to approach one of them, generally the ball in connection with the inner coating of the jar (positive), to which it is drawn. It there receives a charge, and is immediately repelled towards the other ball, where it deposits its charge and also a portion of its natural share, so that its contact with the

outer ball renders it electro-negative, in which condition it is repelled from that ball, and at the same time attracted by the positive one, where it again receives a charge, and is again repelled; and so on, making a series of rapid journeys from ball to ball, until it has carried so much electric fluid from the inner to the outer surface of the jar, that the forces become too feeble to keep it in motion any longer; but it will sometimes play between the two balls of the jar for a quarter of an hour.

If, instead of a ball, a small bell in connection with the outer surface of the jar, and, instead of a gilt pith ball, a small hollow brass ball be suspended by a silken thread, you may have a musical discharge of the jar, by a ringing of the bell. Some jars are fitted up with two bells, one on the top, in connection with the inner surface, and the other in connection with the outer surface; but they neither receive so high a charge, nor do they keep the pendulous ball long in play, for the edges of the positive bell discharge the fluid rapidly to the surrounding air.

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*An Account of a New Electrometer, contrived by Mr. Wm. Henly, and of several Electrical experiments made by him; in a Letter from Dr. PRIESTLEY, F.R.S., to Dr. Franklin, F.R.S.\**

In my history of Electricity, and elsewhere, says Dr. Priestley, I have mentioned a good electrometer, as one of the greatest desiderata among practical electricians; to measure both the precise degree of electrification of any body, and also the exact quantity of a charge before the explosion, with respect to the size of the electrified body, or the jar or battery with which it is connected; as well as to ascertain the moment of time in which the electricity of a jar charges, when, without making an explosion, it is discharged by giving it a quantity of the contrary electricity. All these purposes are answered, in the most complete manner, by an electrometer of this gentleman's contrivance, a drawing of which I send you along with the following description.

The whole instrument is made of ivory or wood, exhibited in fig. 5 plate vi. ; *b*, is a cork ball at the lower end of an exceedingly light rod, which is made to turn on the centre *a*, of a semi-circle *c*, and so as always to keep pretty near its limb, which is graduated: it is supported by a stem, and may either be fixed to the prime conductor, or be let into the brass knob of a jar or battery, or set in a stand, to support itself.

The moment this little apparatus is electrified, the light rod, with its ball *b*, is repelled by the stem, and consequently begins to move along the graduated edge of the semicircle *c*; so as to mark, with

\* Philosophical Transactions, p. 359, Original: or p. 323, Hutton's Abridgement.

the utmost exactness, the degree in which the prime conductor, &c. is electrified, or the height to which the charge of any jar or battery is advanced: and as the materials of which this little instrument is made are very imperfect conductors, it will continue in contact with any electrified body, or charged jar, without dissipating any of the electricity.

If it should be found by trial, in the dark, that any part of this instrument contributes to the dissipation of the electric matter, which, when the electrification was very strong, I once observed mine to do, it should be baked \* a little, which will presently prevent it. If it be heated too much, it will not receive electricity readily enough; and then the motion of the index will not correspond with sufficient exactness to the degree to which the body to which it is connected is electrified: but this inconvenience is easily remedied, by moistening the stem and the index, for the semicircle cannot be too dry.

I find, by experience, that this electrometer answers all the purposes I have mentioned, with the greatest ease and exactness. I am now sure of the force of any explosion before a discharge of a jar or battery, which I had no better method of guessing at before than by presenting to them a pair of Mr. Canton's balls, and observing their divergency at a given distance; but the degree of divergency was still to be guessed at by the eye, and the balls can only be applied occasionally; whereas this instrument, being constantly fixed to the prime conductor or the battery, shews, without any trouble, the whole progress of the charge; and, remaining in the same situation, the force of different explosions may be ascertained with the utmost exactness before the discharge.

If a jar be loaded with positive electricity, and I want to know the exact time when, by attempting to charge it negatively, it first becomes discharged, I see every step of its approach to this state by the falling of the index; and the moment I want to seize is the time when it has got into a perpendicular situation, which may be observed without the least danger of mistake. Accordingly, I find that in this case not the least spark is left in the jar. If I continue the operation, the index, after having gained its perpendicular position, begins to advance again, and thereby shews the exact quantity of the opposite electricity that it has acquired.

Considering the admirable simplicity, as well as the great usefulness of this instrument, it is something surprising that the construction should not have occurred to some electrician before this time. Nollet's and Mr. Wart's invention of threads, projecting shadows on a graduated board, resembled this apparatus of Mr. Henly's, but was a poor and awkward contrivance in comparison with it; nor was Richman's gnomon, though a nearer approach to this construction,

\* Warmed a little, to dry off the damp, particularly from the index.—*Original.*

at all comparable to it; and the ingenious author of it had no knowledge of either of these methods when he hit on this.

Many of the effects of my battery, by breaking glass, and tearing the surface of bodies, Mr. Henly's performs by a single jar, only increasing the weight with which the bodies are pressed, while the explosion is made to pass close under them. By this means he raises exceedingly great weights, frequently six pounds troy, and shatters strong pieces of glass into thousands of the smallest fragments; he even reduces thick plate glass by this means to an impalpable powder. But what is most remarkable is, that when the pieces of glass are thick and strong enough to resist the shock, they are marked by the explosion with the most lively and beautiful colours, generally covering the space of about an inch in length and half an inch in breadth.

In some of the pieces which he was so obliging as to send me, these colours lie all intermixed and confused; but in others I observe them to be disposed in prismatic order, in lines parallel to the course of the explosion, and in some I have counted three or four more distinct returns of the same colour. He has lately informed me that since he sent me this piece he has struck these prismatic colours into another mass of glass, in a still more vivid and beautiful manner, the colours shooting into one another. This effect, he says, was produced by making a second explosion, without moving any of the apparatus after the first. When the glass in which these colours are fixed is examined, it is evident that the surface is shattered into thin plates, and that these give the colours, the thickness of them varying regularly as they recede from the path of the explosion.

Besides these improvements, Mr. Henly has, likewise, in a very ingenious manner, diversified several of the more entertaining experiments in electricity, particularly in his imitation of the effects of earthquakes, by the lateral force of explosions; and he has also hit on several curious facts that, unknown to him, had been observed before by others. The following particular, however, I believe is new: exciting a stick of sealing wax, and using a piece of tin foil for the rubber; he found that it would electrify positively as well as glass rubbed with silk and amalgam.\*

\* Mr. Canton had shewn this fact long before, or at least he shewed that sealing wax became electro-positive by being rubbed with metal. See Phil. Trans. vol. liii.—EDIT.

## MISCELLANEOUS.

## LIVERPOOL MECHANICS' INSTITUTION.

The Liverpool Mechanics' Institution cost no less than £15,000 : contains upwards of 3,300 members ; 850 pupils in three day schools ; 600 pupils in fifteen or sixteen evening classes ; has 50 teachers regularly employed, whose salaries amount to £5,000 a year ; a library of 7,000 volumes, with 1,300 readers, and a daily distribution of 200 volumes ; and public lectures twice a week, attended by audiences varying from 600 to 1,300.

## IMPORTANT INVENTION IN MACHINERY.

Our attention has been called, by one of the provincial newspapers, to a very important mechanical invention, for which a patent has recently been granted to two Scotch gentlemen, and which promises to effect a greater and more beneficial change in the working of machinery than has taken place since the brilliant discoveries of Watt. The two great draw-backs of the steam engine (besides the large space it occupies) have been the bulk, and the consequent expense of the fuel which it requires, the latter preventing its application to many purposes for which it would otherwise have been a most effective agent ; and the former impeding its locomotive energy, and confining it, particularly as regards navigation, within comparatively narrow limits. Any reduction in either of these respects is thereby obviously so much clear gain. The invention to which we allude promises to effect a prodigious saving in both, by diminishing the expenditure of fuel to somewhat less than one-fifth of what is now required for an equal degree of power. It has already, we are informed, been put to the test by the construction of an engine of about 20 horse power, which has for some time been driving all the machinery of an extensive foundry with no larger consumption than we have just mentioned, and with every prospect of a considerably greater reduction being effected by some slight changes in the details. The motive power used is the common atmospheric air ; and another great advantage of the new engine arises from a saving of space equal to what is usually occupied by the furnace and boilers of a steam-engine. If these things be so, it is impossible to calculate the results to which so important a discovery must lead.

THE ANNALS  
OF  
ELECTRICITY, MAGNETISM,  
AND  
CHEMISTRY;  
AND  
GUARDIAN OF EXPERIMENTAL SCIENCE.

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NOVEMBER, 1841.

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*GALVANOPLASTIK; or the Process of Cohering Copper into Plates, or other given forms, by means of Galvanic Action on Copper Solutions.* By DR. M. H. JACOBI, Privy Councillor to the Emperor of Russia, and Member of the Royal Academy of Sciences of St. Petersburg.\*

(Continued from page 328.)

2.

We have hitherto said, for example, that zinc and copper have this power of galvanic action; but it is to be understood, that a similar action is produced in a greater or less degree by a combination of any pair of dissimilar metals; and experiment has shown that they observe a certain uniform relationship, according to that represented by the following series, which we have introduced in the usual way:—

PLATINUM.  
GOLD.     •  
SILVER.  
QUICKSILVER.  
COPPER.  
LEAD.     •  
TIN.  
IRON.  
ZINC.

In this series the metals are placed according to their electrical characters, in the same relation to each other as zinc has to copper; so that any one of them operates as *zinc* to all those above it, not however in point of energy, but only of the kind and manner of action; and the more distant from one another any two metals

\* Translated from the German Edition.

*Ann. of Elec.* Vol. VII. No. 41. Nov., 1841.

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stand in the series, the greater the galvanic action they will develop. Hence, a galvanic series of platinum and zinc is more powerful than one of copper and zinc; and the latter, again, more powerful than one of lead and zinc, &c. It is not to be understood, however, that the power of any two metals in the table depends upon the number of intermediate ones, because a galvanic series of platinum and iron is much feebler than a series of copper and zinc; although in the former case there are six intermediate metals, and in the latter there are only three. Platinum and gold, or gold and silver, or silver and copper, combined together in a voltaic series, give but very feeble action. I must here mention, however, that the behaviour of the metals towards one another is, in some cases, altered by the employment of several kinds of liquid for the exciting medium; but this is to be considered only as an exception, which does not frequently come into operation.

Besides these principal metals, there are other substances, which, as regards their galvanic effects, are very considerable. Of these, however, the principal, in a practical point of view, are charcoal and plumbago; but the latter can only be considered as a peculiar quality of charcoal, usually containing a portion of iron. Both these substances stand higher in the scale of electric bodies than platinum, so that a galvanic series of plumbago and zinc is the most powerful hitherto known.

Before proceeding further, I will avail myself of this opportunity of addressing myself to the industrious part of my readers. You have seen, in the introduction to this work, that to you this science is capable of performing a very important service, which it performs in a very disinterested manner; it expects, however, that you, in return, will let no opportunity pass unnoticed in which you can be useful to its promotion. By this means you at once rank amongst the best and most useful of your fellow-citizens. On this account I have here mentioned the excellent properties of graphite, in reference to galvanic action. It is difficult, however, to procure this substance in large masses or plates; it is, therefore, generally produced by some artificial means. It may be formed by smelting it with iron in a highly-heated furnace, by which means we find it in the character of cast-iron; which, as is well known, is but another combination of iron and carbon. We have only other experiments to make, adding more carbon than is usually contained in cast-iron, to arrive at a metal whose galvanic properties would be equal to those of platinum.

Such cast-iron, in consequence of its containing so great a quantity of carbon, might possibly be but of very little service for any other purpose. In galvanic apparatus, however, as no great resistance has to be withstood, the compound would not require to be of any peculiar firmness of cohesion. A compactness sufficient for it to be cast into plates, or perhaps, cylindrical vessels, might be all that would be required. It might be preferable to give it such a



property that none of the graphite would be acted on by the nitric or the sulphuric acids, whether concentrated or diluted. It is well known that the object in view may be obtained by a species of cementation; or by remelting cast-iron, with additional carbon, in closed vessels. In the laboratory of the chemist or physician, the difficulties and inconveniences to be met with in such a course of experiments, would be too great for the undertaking; but in the iron-foundery, where every requisite is at hand, the good-will of the proprietors, or of the managing officers, would be the only additional trifle required. Should any person find this statement of facts sufficiently interesting to induce him to proceed to experiment, and that he succeeds in forming a mixture of iron and carbon that will become serviceable for galvanic purposes, it would be satisfactory to me to have this proof obtained; even on my own account it would be gratifying to have my views submitted to the requisite trial.

But were such an electric property as this to be met with in a metallic mixture or alloy, we could not, from that circumstance alone, establish any general rule for facilitating the pursuit of still better compounds, whilst furnished with only a few experiments; for it must be borne in mind that the electrical character of the alloys does not always take a place between those metals of which they consist, but more frequently they stand either much higher or much lower in the series. Such, for instance, is the case with brass, which mostly acts, in galvanic arrangements, either quite as well or even better than copper, which is one of its constituents. On the other hand, also, either amalgamated zinc, or a compound of zinc and quicksilver, even better than zinc alone, although the quicksilver alone stands high in the galvanic series. And I have discovered a still better compound than quicksilver and zinc, which consists of 38 parts of quicksilver, 22 parts of tin, and 12 parts of zinc. Nevertheless, in such alloys as these, where too much quicksilver is introduced, the disadvantage is, that they are extremely brittle, and have but little coherency.

### 3.

The following are the most essential circumstances by which the power of galvanic arrangements is influenced:—

1. The size of the plates as regards their surface.
2. The distance at which they are placed from each other in the liquid.
3. The strength and character of the exciting acid or saline solution.
4. The character and the length and thickness of the conductor which unites the zinc and copper plates.
5. The extent of the series, or number of pairs of plates contained in the battery.

Before we consider these circumstances more particularly, we will just touch upon some of the various phenomena which are shown

by the galvanic powers themselves. Upon this point we know nothing further than what has hitherto been stated, excepting that we observe that the water of the exciting liquid in the battery becomes decomposed; that the hydrogen is developed at the copper plate; and that the zinc plate, or whatever is employed in lieu of it, dissolves in the liquid; and furthermore, that these operations take place only when the metallic plates are connected with each other by a good metallic conductor, usually called the connecting wire or the connecting bow; for as soon as the connection in any part of the circuit is interrupted, all galvanic action ceases.

As the galvanic activity terminates at the precise moment that an interruption takes place, it may be necessary to say a few words on that point.

Since, then, the uninterrupted conductor joining one plate to the other is the first condition of galvanic energy, it will be necessary to say a few words on that point. We will suppose, provisionally, that this conducting medium is metallic, and that it may be permitted to consist of wires or strips of various dimensions, and also of different kinds of metals; supposing in all cases that they are intimately bound, or otherwise fixed together in good metallic connection. This end is obtained very conveniently by means of solder; but it is sufficient that the ends of the conductors be made bright, and either twisted or rivetted together. If we are desirous of having a contrivance by means of which we can break and make contact with still more convenience, the clamp screw, (binding screw,) or small cups holding mercury, may be employed advantageously. The cups may be made of any convenient size, from hard wood or glass, or even pasteboard, and fashioned in any way the experimenter thinks proper. If metal be used for this purpose, it must be either copper or iron. Brass, in time, corrodes by the quicksilver, and lead and tin are shortly destroyed by its action; hence the iron or copper cups should not be soldered with soft solder, the hard solder alone being suitable for this purpose.\*

Fig. 2 is a cup for holding quicksilver, and can be screwed firmly and immediately to a wire which is soldered to another plate of a galvanic apparatus. This figure needs no further explanation; and even but very little will be wanted to understand figures 3, 4, 5, and 6; where 3 represents a *single* clamp screw soldered to a wire; and the other, *double* clamp screws similarly connected with other wires. A broad strip of copper soldered to a clamp screw, as represented by figure 7, is preferable to wires; *a, b*, is a small brass or copper plate, in the middle of which it carries a small winding screw pin, partly seen above. The strip *e, f*, must have a hole or

\* This remark would be perfectly useless when the cups are made out of solid pieces of metal, as is the custom in this country. I have used brass cups for many years, which are still as good as ever. They are amalgamated inside, which is a convenience, by preventing the mercury from being thrown out by those accidental jolts which so frequently dislodge it from wooden and other cups, to which it will not adhere.—TRANS.

slit in the middle to admit the vertical screw pin, and then it is firmly screwed down by the nut. For preparations of this kind it will be advisable to have a good stock in store, and to have the binding screws with perforations of different dimensions, suitable to the various dimensions of the conducting wires that it may be found necessary to use and clamp together. The screw heads should also be both milled and cut for the introduction of a turn-screw, if found necessary. The milled head will be found very convenient for the first advances of the screw, which, in many cases, will require no aid whatever from the screw-driver.

In referring again to figure 1, which represents a simple galvanic circle, I will only remark, that we consider the galvanic energy to consist in a constant current of electricity, which sets out from the zinc plate to the acid solution, through which it passes to the copper plate; and from the copper it traverses the metallic conductor back again to the zinc, in the manner represented by the small arrows in the figure; and that we shall frequently avail ourselves, as we proceed, of the expression *galvanic* or *electric* current. Hitherto we have said nothing on these close circles, further than that there is a developement of gas on the surface of the copper plate; that the zinc plate dissolves without effervescence; and that these effects immediately cease on the circle being interrupted, either by lifting one end of the conductor out of the quicksilver in the cup, or by any other means whatever. We can, however, observe some other very remarkable effects by the action of such a closed circle.

Most of my readers are well aware that a magnetic needle, freely suspended, either upon a pin point or by means of a delicate fibre, directs itself nearly north and south. If we now consider that the conducting wire, which joins the copper and zinc plates in figure 8, is extended in the natural direction of the magnetic needle, N. S., we observe, whilst the circle is closed, that the needle turns aside from its first position, and that it immediately returns again when the circle is broken. The direction of this deviation is various, accordingly as the needle itself is placed *above* or *below* the conducting wire; and also, accordingly as the current traverses the wire from north to south, or from south to north. Figures 9, 10, 11, 12, explain the kind and manner of these deflections sufficiently to render it almost needless to add, that figures 9 and 10 represent them when the wire is *above* the needle, and figures 11 and 12, when the wire is beneath it.

It is an invariable rule, that the greater the deflection of the needle, the stronger is the galvanic power, or the more powerful is the electric current, which also stands in near relation to the quantity of the hydrogen liberated in a given time, or to the quantity of zinc dissolved. The magnetic needle is, therefore, a valuable instrument, by which the mere inspection of its deflections informs us of the energy of galvanic force. It is principally for this purpose that we shall, further on, make use of it: for the present we cannot

dwell any longer on these highly remarkable electro-magnetic phenomena.

The next observation that we shall make on the connecting wire, is that of its becoming heated as soon as it enters into the galvanic circle. This heating power is greater in some proportion to the increase of the energies of the battery ; but, at the same time, it depends, also, on the size of the conductor, and on the character of the metal of which it consists. The circumstances which prevail in the production of heat by galvanic arrangements are considerably entangled in complexity, and all that we can state in this place is, that with the common metals, a wire of platinum generally becomes heated to the greatest extent ; and if it be not too long or too thick, it is easily brought to the highest glowing heat, and even fuses. In consequence of the heat becoming greater as the galvanic power increases, we are enabled to ascertain the power of the battery by the degree of glow produced in a thin platinum wire placed in the circle, though not with such precision as by the deflections of a magnetic needle.

Figure 13 represents a contrivance for heating the platinum wire, and requires but little explanation, further than that it consists of two stout cotton or silk covered wires, which are united at the upper extremities by a fine platinum wire of about half a foot long ; and that, when used, the lower bright ends, *x*, *y*, are either *directly* or *indirectly*, by means of other wires, brought into connection with the plates of the battery. I will also remark, that it is convenient to have such *test wires* of various lengths and thicknesses, by means of which the power of any determined size of battery may be measured, and its required working order duly ascertained. I will only add, in this place, that a chemical energy may also be ascertained by means of a galvanic current ; on which topic I shall treat pretty copiously further on, because it is this that forms the foundation of the useful part of the application of our subject.

In a former part of this work we have considered the galvanic energy under the form of a current, which proceeds with an uninterrupted circulation from the zinc, through the acid liquor, to the copper, and through the conducting wire from the copper back again to the zinc. The power which sets this current into motion depends on the natural electric condition of the effective metals of which the battery consists ; and, as we have seen above, (Art. 2.) this power is always greater as the metals are more distant from one another in the tabulated series, (page 337.) But this power experiences certain obstructions, or, as we frequently say, *Conduction resistance*, (*Leitungswiderstände*), which weaken the strength of the currents. If, now, we pursue an electric current along its channel, we shall find the *first* resistance that it experiences at the plane of contact, where it passes from the zinc to the liquid in the battery. A *second* resistance is experienced whilst passing through

that liquid ; a *third* when the current leaves the liquid to enter the copper plate ; and a *fourth* in the conducting wire itself. All these resistances being added together and brought to a common unit, we have, by many experiments and observations, obtained the following principal law :—*The strength of the currents diminishes as the sum of the operating resistances increases* :—So that, when this sum is double, the strength of the current is reduced to one-half of the original standard ; and when the resistances are but one-half of the first unit, the strength of the current is doubled, &c. The determination of these individual resistances, however, is very difficult, and all that has been acquired is to obtain a general survey to which we can have some practical reference.

From the experience hitherto obtained the following laws have been determined :—

1. That all the resistances to transmission become *smaller* as the surfaces of the plates are enlarged.

2. The resistance presented by the liquid in the battery becomes *greater* as the plates are more distant from one another ; and *smaller* the greater the transverse section of the liquid. This resistance also depends on the natural character of the fluid conductor ; and, in general, it becomes *smaller* as the acid, or saline solution, employed in the battery, is more concentrated.

3. The resistance of the conducting wire likewise depends upon the character of the metal of which it consists, and operates, moreover, *in proportion to its length, and inversely as the transverse section*, whether it be a wire or a strip.

We thus see that the power of a single pair of plates can be strengthened only by lessening these resistances : which can only be accomplished by increasing the transverse dimensions of the conducting wire, enlarging the plates, and bringing them nearer to each other, and, finally, by strengthening the liquid of the charge. With the two latter circumstances we soon arrive at certain limits. For instance, the plates must not be permitted to touch one another in the liquid ; and there are also good reasons to suppose that they ought not to be placed nearer to each other than about a quarter of an inch. There are also disadvantages by employing too strong a charge, by facilitating crystallization when the fluid is saline and too saturated, in which case the action becomes diminished. If concentrated sulphuric acid be used in the battery, we obtain scarcely any action ; but if the acid be diluted with from four to five parts of water, the action becomes very great, &c.

It appears from hence, that when the resistance of the conducting wire is not too great, the best means to increase the power is to enlarge the plates ; which we will endeavour to illustrate by an example. If we allow the power which sets a current in motion from a single pair of copper and zinc, of one square inch surface, to be represented by the number 1, or as an unit of force, the resistances of that pair of plates themselves may be equal to the

resistance presented by a copper wire of 4000 feet long and one tenth of an inch thick. Moreover, if this single circuit be closed by a conducting wire of 1000 feet long and also one tenth of an inch thick, the power of such a pair of plates will, therefore, be as the fraction  $\frac{1}{4000 \div 1000} = \frac{1}{5000}$ . If, now, the conducting wire be shortened to about one foot long, the power will be increased to  $\frac{1}{4001}$ ; and, finally, when the plates, by means of solder, are immediately joined together, the power becomes increased to  $\frac{1}{4000}$ . By shortening the conducting wire in this case, the power is brought from  $\frac{1}{5000}$  to  $\frac{1}{4000}$ , or increased about one-fourth.

If we increase the size of the plates to 10 inches a side, so as to have 100 square inches of surface, the power obtained is  $\frac{1}{40 \div 1000} = \frac{1}{1040}$ . Had the plates 20 inches a side, or 400 square inches of surface, we should obtain a power of  $\frac{1}{1010}$ ; and if they were 80 inches a side, or 6400 square inches of surface, the power would be  $\frac{1}{1000 \div 4}$ , or nearly  $\frac{1}{1000}$ . We learn from these data, that by increasing the magnitude of the surface the power advances as far as  $\frac{1}{1000}$ , or rises to a fivefold extent. But in order to strengthen the power of the current, we must not only increase the extent of the surface of the plates, but, at the same time, shorten the conducting wire also. Hence, for instance, we obtain a hundred-fold current by 10-inch plates and a conducting-wire of 10 feet long; a four-hundred fold current by 20-inch plates and a conducting wire of  $2\frac{1}{2}$  inches long, &c. Indeed, the power rises as decidedly by shortening the conducting wire as by augmenting the surface of the plates. We must, therefore, not be surprised that little or no power is gained by an increase of size in the plates, when the conducting wire is long and thin. If, however, circumstances be such, that the connecting wire must be 1000 feet long, and the battery consists of ten-inch plates, we may augment the power a hundred-fold by using 100 such wires side by side for the conductor, or by closing the circuit with one wire 1000 feet long and one inch thick.

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*Abstract of a Letter to Baron A. Humboldt, upon the Invention of the Mariner's Compass.* — Lettre à M. le Baron A. de Humboldt, sur l'Invention de la Boussole; par M. J. KLAPROTH. Paris: 1831. pp. 138.

Read before the Connecticut Academy of Arts and Sciences, by EDWARD E. SALISBURY, A.M., and published by permission of the Academy.

This is the title of a little volume, published six years ago, in which M. Klaproth, a well-known orientalist, since deceased,\* has

\* M. Klaproth was a Prussian, born at Berlin in 1783, and died at Paris in 1835.

given the result of researches made by him, respecting the invention of the mariner's compass.

It has been long since generally admitted, that the classic writers, though they had some idea of the attracting and repelling power of the magnet, were ignorant of its polarity, and consequently of its applicability to navigation. But the later opinion, that the merit of this discovery is to be attributed to an Italian of the middle age, must be also abandoned. Klaproth's investigations go to prove, that our knowledge of the magnet, as well as of the magnetic needle and compass, has been derived, either directly or indirectly, from the East, and originally from China, where the earliest notices of both belong.

Should this work not have become known already in this country, a brief abstract of its most important points may not be unenterprising or without value.

The name *magnet* comes from the Greek. The most ancient Greek name for this natural production was λίθος ἡρακλεία, *stone of Heraclea*, a city situated at the foot of Mt. Sipylus, in Lydia. This city was afterwards called Magnesia, and the name of the stone, for which it was remarkable, became changed to *Μάγνησιος, λίθος, stone of Magnesia*, or vulgarly, *Μάγνης*, and *Μάγνητης*. The same name is found in the Latin, and its origin from the Greek is confirmed by Lucretius, who says—

“Quem magneta vocant patrio de nomine Graii :  
Magnetem, quia sit patriis in montibus ortus.”

Other languages into which the name *magnet* has been incorporated, are the modern Greek, (*Μάγνητης*), the German, (*magnet*), the Hollandish, (*magneet-stein*), the Danish, (*magneet*), the Swedish, (*magnet*), the language of the Grisons, in the dialect of Surselva, (*magnêt*), the Hungarian, (*magnet-kö*), the Russian, (*magnêt*), the Polish, (*magnes, magnet*, and *magnet-kamien*), and the Vendish of Styria, (*magnet*.) where, excepting in the modern Greek, the Latin has unquestionably been the medium of transporting the word from the ancient Greek.

Another name in use in several European idioms, as in the Greek, Italian, French, the Romance language of Surset, the Bosnian, Croatian, and the Vendish of Styria, is *calamita*. This word appears to be of Greek origin, and is given by Pliny as the name of a *small green frog*. The application of the term to the magnet is explained by a fancied resemblance to that animal in the magnetic needle, when poised on water by means of small reeds, projecting beneath it like the legs of a frog in motion, according to the usual mode in early times, in Europe, of adapting it to the mariner's use. But that the idea of such a resemblance was not original in Europe, one might be led to suspect, from the analogy of the Birman name for the compass *anghmyaoung*, which signifies *lizard*, and will be rendered still more probable by evidence, here-

after to be given, that this mode of using the magnetic needle in navigation, was adopted in China about eighty years previous to the earliest mention of the needle itself in any European writer.

Many of the terms applied to the magnet, both in European and Asiatic languages, allude to one or another of its characteristic properties. Among these, the French *l'aimant*, the lover, and the Spanish and Portuguese *iman*, with the same signification, is particularly worthy of notice, as having its precise correspondent in the Chinese *thsu chy*, of which a celebrated Chinese naturalist, who flourished in 1580, observes: "If this stone had not a love for the iron, it would not make it come to it," and a writer of an age eight centuries earlier: "The magnet draws the iron like a tender mother, who causes her children to come to her, and it is for this reason that it has received its name." In India, also, the magnet was of old personified as capable of tender attachment, in the Sanscrit nome *thoumbaka*, the kisser, from which are derived several appellations now in use in that country, as *tchoumbok* in the Bengalee, and *tchambak* in the Hindostanee. Another Sanscrit term for the magnet is *ayaskānta*, the loved stone, or *ayaskānta-mani*, the stone loved by iron, which also the Bengalee retains; and in the Cingalese the magnet bears the name of *kāndakô-galah*, the stone that loves, which is apparently composed of the Sanscrit *kānta*, loving, and the Cingalese *galah*, stone.

The languages of Mussulman Asia derive the names which they give to the magnet mostly from the Greek *Μάγνητις*: thus in Arabic we find *al-māghnāthis*; in Persian *seng-i-maghndāthis*—the stone maghnāthis; and in Turkish *miknāthis*.

Of the names given to the magnetic needle and compass, one which is to be met with in many European languages, is the Italian *boussola*, the Portuguese *bussola*, the Spanish *brujula*, the French *boussole*, &c. Some Italian authors have claimed this term as original in their own language, and have sought to argue, from its having been so widely adopted in other languages, in favour of their national assumption of the honour of having invented the compass. The word cannot, however, be deduced from an Italian origin any more reasonably than from an assumed English diminutive *bozel*, no such diminutive existing, which some writers have attempted; nor does the Greek *Μησσάλας* bear the appearance of being original with that language. The derivation of both the Greek *Μησσάλας* and the Italian *boussola*, and so of the corresponding words in other languages of Europe, is to be found in the Arabic *mouassala*—arrow, an initial *m* of Arabic words having been very commonly changed, in the middle age, to *b*. *Mouassala* is itself one of the names given to the magnetic needle in Arabic.

Among the Turks and Persians, the term for the compass, in most general use, is *kiblêh-namêh* or *kiblêh-numa*—indicator of the *kiblêh*, which is the direction to be faced in prayer, and consequently, as Mecca lies to the south of most of the Mohammedan



countries, *the south*. With this is perfectly synonymous the Chinese appellation *tchi nan*—*indicator of the south*, and the Manchow *dchoulesî dchorikoû*, for the magnetic needle. The Persians undoubtedly derived their name for the compass, *kiblêh-namêh*, from the Chinese, for it is a peculiarity limited to the Chinese and those who have adopted their civilization, that they make the south their principal pole, regarding this as the anterior and the north as the posterior side of the world; according to which they also place the throne of their Emperor, and the principal façade of their edifices, so as to front the south.

As the Hindoos have never been addicted to navigation, the knowledge of the compass seems to have been introduced but very late among them, and the names they give to it are for the most part foreign. In the English provinces of India it is called *compass*, from the English; and in the Cingalese of Ceylon, *kompâ souwa*, a corruption of *compass*. The Hindostanee has adopted the Persian term *kiblêh-numa*—*indicator of the south*.

In these comparisons of the most current terms for the magnet and the magnetic needle and compass, in the eastern and western world, there is not a little to lead one to believe that the discovery of the wonderful properties of the magnet originated in the remote Orient, and was gradually communicated to the nations of the west. But there are historic notices of the magnetic needle and compass, which also point to the east as the field of the first discovery of the polarity of the magnet and its applicability to navigation.

The earliest explicit mention of the magnetic needle, by any European writer, is in a poetical work of Guyot de Provins, dating about the year 1190. The next as to date is found in the *Historia Orientalis* of Jacques de Vitry, referring to the year 1204: "Adamus in Indiâ reperitur—ferrum occultâ quâdam naturâ ad se trahit. Acus ferrea, postquam adamantem contigerit, ad stellam septentrionalem, quæ velut axis fermamenti, aliis vergentibus, non movetur, semper convertitur, unde valdè necessarius est navigantibus in mari." It would be difficult to give any authority to this passage, and not recognize the east as the source of knowledge, among Europeans, of the polarity of the magnet. Not long before the year 1260, Brunetto Latini, "maître du divin Dante," being on a journey in England, saw the magnet and the magnetic needle for the first time, in visiting Roger Bacon, and a fragment of a letter of his, written on the occasion, which has been preserved, describes them thus: "He shew me the magnet, a disagreeably looking black stone; it readily unites with iron; a small needle is taken into the hand and fastened in a bit of reed, then it is put upon a surface of water, and one stands over it, and the point turns towards the star (the polar star); in case the night is obscure, and neither star nor moon is seen, the mariner may keep to his right course."

Albertus Magnus, of Swabia, who flourished about the middle of the thirteenth century, quotes in a work of his, "*De Mineralibus*,"

a passage from a "treatise concerning stones," attributed to Aristotle, of which the following portion merits particular attention: "Angulus magnetis cujusdam est, cujus virtus apprehendi ferrum est ad *zoron*, hoc est, septentrionalem, et hoc utuntur nautæ. Angulus verò alius magnetis illi oppositus trahit ad *aphron*, id est, meridionalem: et si approximes ferrum versus angulum *zoron*, convertit se ferrum ad *zoron*, et si ad oppositum angulum approximes, convertit se directè ad *aphron*." Vincent de Beauvais, a cotemporary of Albertus Magnus, has left a similar passage, likewise quoting Aristotle, in his "*Speculum Naturale*:" "Angulus quidem ejus cui virtus est attrahendi ferrum, est ad *zaron*, i. e. septentrionalem, angulus autem oppositus, ad *aphron*, i. e. meridiem. Itaque proprietatem habet magnes, quod si approximes ei ferrum ad angulum ipsius qui *zaron*, i. e. septentrionem respicit, ad septentrionem se convertit, si verò ad angulum oppositum ferrum admoveris, ad *aphron*, i. e. meridiem se movebit." The names given in these two passages to the north and south pole, *zoron* or *zaron*, and *aphron*, are the Arabic words for north and south. As to the work here attributed to Aristotle, under the title of a "treatise concerning stones," we have no such Greek text of this author, and it is doubtful if ever he wrote such a book. There is an Arabic treatise entitled *The Book of Stones*, composed by a certain Lucas, son of Serapion, but purporting to be a translation from Aristotle, which Baron de Sacy has shewn to be the true source of citations under the name of Aristotle, in the writings of Tëifachi and Bëilak Kibdjaki; and very probably Albert and Vincent have quoted this same work in their account of the polarity of the magnet. However, the names *zoron* and *aphron*, applied by these authors to the two magnetic poles, are sufficient to prove that they derived their knowledge of the magnet's polarity from an oriental source.

But there is no evidence that the Arabs were the inventors of the magnetic needle. It may, indeed, have been in use among the Arabian navigators, before it was noticed by men of science; but we have in no Arabic work any mention of it which goes back beyond the year 1242. In this year, Bëilak Kibdjaki made a voyage from Tripoli to Alexandria, and in his treatise entitled *The Treasure of Merchants, touching the knowledge of stones*, he has recorded his observations on that occasion, respecting the magnetic needle. "As to the properties of the magnetic needle," he writes, "it is to be observed, that the captains who navigate the sea of Syria, when the night is so obscure that they can see no star by which to steer their course according to the four cardinal points, place a pitcher full of water in the interior of the vessel, to be sheltered from the wind, and then take a needle and pass it through a piece of wood or reed, forming a cross, which they throw into the water in the pitcher prepared for the purpose, and it floats. They then take a magnet large enough to fill the palm of the hand, or smaller, and bring it near the surface of the water, giving their hands a rotatory

movement towards the right, so that the needle turns about on the surface of the water. Then they withdraw their hands all of a sudden, and truly the needle points north and south. I myself saw this done on my voyage from Tripoli, in Syria, to Alexandria, in the year 640," (or 1242 of our era.) "They say," he continues, "that the captains who navigate the Indian ocean supply the needle and piece of wood by a sort of fish, of thin iron, hollow, and so made with them, that, when thrown into the water it floats, and shows by its head and tail the two points of north and south." So early, then, as the year 1242, the water-compass was in general use on the Syrian waters, and was known, it is to be presumed, as well to Arabian as to European navigators. But what this author, Bēilak, says of the peculiar form, according to report, of the magnetic needle which was used in the Indian ocean, indicates an independent knowledge of it in that quarter of the globe; and recalling the signification of *calamita*, *little green frog*, and the Burman appellation for the compass, meaning *lizard*, leads one to look further to the east than any of the Mohammedan countries for the original discovery of the polaric properties of the magnet. We shall presently see that between 1111 and 1117, the Chinese made a water-compass exactly such as Bēilak describes that which he saw in 1242, in the Syrian waters, and also like that which Jacques de Vitry saw within the first half of the thirteenth century, in the possession of Roger Bacon.

The Chinese have been acquainted with the magnet and its attractive force and polarity from the highest antiquity. In a Chinese dictionary, composed in 121, by Hiu-tchin, the magnet is mentioned as a "stone with which one can give direction to the needle." About a hundred years later, as we learn from P. Gaubil, in his history of the dynasty of the Tháng, there is found a distinct notice of the compass as an instrument by which to ascertain the points of north and south. Under the dynasty of the Tsin, (i. e. between 265 and 419,) Chinese vessels were already steered to the south by means of the magnet. But the Chinese were acquainted with the declination of the needle, also, a long while before it was supposed to be first discovered by Columbus. In a medical natural history, composed between the years 1111 and 1117, the author gives the following notice of the magnet and of its properties. This is the most ancient *description* of the magnet found as yet in any Chinese book: "The magnet is covered over with little bristles slightly red, and its superficies is rough. It attracts iron, and unites itself with it; and for this reason it is commonly called 'the stone which licks up iron.' When an iron point is rubbed upon the magnet, it acquires the property of pointing to the south; yet it always declines eastward, and is not perfectly true to the south. On this account, a thread of new cotton is taken and attached by a particle of wax as large as a mustard-seed, exactly to the middle of the iron, which is thus suspended in some place where there is no wind. The

needle then points, without variation, to the south. If the needle is passed through a little tube of thin reed, which is afterwards placed on water, it directs to the south, but always with a declination to the point *ping*, that is to say, east  $\frac{1}{2}$  south." The accuracy of this observation, referring it to the capital city of the empire, is confirmed by P. Amiot, who, after taking magnetic observations at Peking for several years, found the variation of the needle there to be constantly from  $2^{\circ}$  to  $2^{\circ} 30'$ .

Upon a due consideration of all these historic data, in connection with the comparison of the European and Oriental names of the magnet, the magnetic needle, and the compass, it cannot appear to any one to be a rash conclusion, that the knowledge of the natural production, as well as of its wonderful applicability in navigation, existed first in China, and was communicated by the intervention of the Arabs to the nations of Europe, probably on occasion of the more frequent intercourse between Europe and the East, to which the Crusades gave rise.

But before the Chinese had applied the magnet to use in navigation, it was employed among them in the construction of *magnetic cars*, by which travellers on land directed their course. Not to cite those stories of the Chinese relative to these cars which lose themselves in a fabulous antiquity, the earliest historic allusion to them dates in the first half of the second century, when the Emperor Tcheou Koung, as it is related, gave to some ambassadors from Tonkin and Cochín-China "five travelling cars, so constructed as always to indicate the direction of the south. The cars which shewed the south," it is added, "always went in front, to shew the way to those who were behind, and to make known the four cardinal points." In the year 235, a Chinese emperor ordered one Makiun to construct a "car which would shew the south," to be deposited in a sort of Museum; and we are informed that the invention had then, for some time, been lost, and was recovered by the ingenuity of Makiun. In a book of annals of the dynasty of the Tsin, the magnetic car of a previous age is thus described: "The figure sculptured in wood, standing upon the magnetic car, represented a genius dressed in feathers. In whatever direction the car inclined or turned, the hand of the genius pointed invariably to the south. When the Emperor went out in form, in his carriage, this car led the van, and served to shew the four cardinal points." From the year 235, the construction of a magnetic car seems to have been a puzzle which different Chinese emperors proposed to the ingenious men of their courts, and the knowledge of the invention appears to have been confined within very narrow limits.

Between 806 and 820, under the Thang dynasty, were first constructed cars called *Kin koung yuan*. These were magnetic cars to which had been added a sort of drum, called *Ki li kou*, a piece of mechanism which may remind one of some curious public time-

pieces still to be seen in old cities of Europe. A *drum-car* is thus described by a Chinese author: "It had two stories, in each of which was a wooden man holding erect a mallet of wood. As soon as the car had run one *ly*,\* the wooden man of the lower story struck a blow upon a drum, and a wheel placed at the middle of his height made one revolution. After the car had run ten *lys*, the wooden man of the upper story struck a little bell."

The *magnetic car* cannot be traced later than 1609. In that year was published a celebrated Encyclopedia, which contains the following passage, accompanying a design of the human figure which was placed upon the magnetic car: "This is a car ornament, of which the dimensions are as follows: It is one foot and four inches in height, and in breadth at the bottom seven inches and four lines. At the extremity of the axle-tree of the car is pierced a round hole of three inches and seven lines in diameter. In this hole moves a peg of the same size, on which is placed the figure of a man sculptured in *jade*,† whose hand always points to the south. This figure had motion in the hole, and turned. In the years Yan Yeou, (from 1314 to 1320,) it was an object to determine the position of the monastery of Yao mou ngan, and the figure on the magnetic car was made use of for this purpose."

*Second Letter to Prof. Faraday, from ROBERT HARE, M.D.,  
Professor of Chemistry in the University of Pennsylvania.‡*

MY DEAR SIR,

39.§ In the month of July last I had the pleasure to read, in the American Journal of Science, your letter in reply to one which I had addressed to you through the same channel. I should sooner have noticed this letter, but that meanwhile I have had to republish two of my text books, and, besides, could not command, until lately, a complete copy of all those numbers of your researches to which you have referred.

40. The tenor of the language with which your letter commences, realizes the hope which I cherished, that my strictures would call forth an amicable reply. Under these circumstances, it would grieve me that you should consider any part of my language as charging you with inconsistency or self-contradiction, as if it could be my

\* A measure of distance variously estimated. John Francis Davis, in his *History of China*, computes thirty *lys* in one English mile.

† A hard stone, of variegated hue.

‡ For Dr. Hare's First Letter to Dr. Faraday, see *Annals*, vol. v., p. 20. Dr. Faraday's Answer at p. 110.

§ As originally printed for the American Journal of Science, the paragraphs of my first letter to Prof. Faraday were not numbered; but as numbers were attached to the paragraphs in the republication of it in the London and Edinburgh Philosophical Magazine and Journal [vol. xvii. p. 44], I have directed them to be attached to this, my second letter, in due succession.

object to put you in the wrong, further than might be necessary to establish my conception of the truth. Certainly it has been my wish never to go beyond the sentiment "*amicus Plato, sed magis amica veritas.*" I attach high importance to the facts established by your "*Researches,*" which can only be appreciated sufficiently by those who have experienced the labour, corporeal and mental, which experimental investigations require. I am, moreover, grateful for the disposition to do me justice, manifested in those researches; yet it may not always be possible for me to display the deference, which I nevertheless entertain. I am aware that when, in a discussion which due attention to brevity must render unceremonious, diversities of opinion are exhibited, much magnanimity is requisite in the party whose opinions are assailed; but I trust that both of us have truth in view above all other objects, and that so much of your new doctrine as tends to promote that end, will not be invalidated by a criticism, which, though free, is intended to be perfectly fair.

41. In paragraph 11 your language is as follows:—"*My theory of induction makes no assertion as to the nature of electricity, nor at all questions any of the theories respecting that subject.*"

42. Owing to this avowed omission to state your opinions as to the nature of electricity, as preliminary to the statement of your "*theory,*" and because I was unable to reconcile that theory with those previously accredited, I received the impression that you claimed no aid from any imponderable principle. It appeared to me that there was no room for the agency of any such principle, if induction were an *action of contiguous ponderable particles consisting of a species of polarity.* It seemed to follow that what we called electricity, could be nothing more than a polarity in the ponderable particles, directly caused by those mechanical or chemical frictions, movements, or reactions, by which ponderable bodies are electrified. You have correctly inferred that I had not seen the fourteenth series of your researches, containing certain paragraphs (38). From them it appears that the polarity, on which so much stress has been laid, is analogous to that which has long been known to arise in a ponderable body, about which the electric equilibrium has been subverted by the inductive influence of the electricity accumulated upon another such body. This is clearly explained in paragraph 4 of your letter, by the illustration, agreeably to which three bodies, A, B, C, are situated in a line, in the order in which they are named, in proximity, but not in contact. "A is electrified positively, and then C is uninsulated." It is evident that you are correct in representing, that, under these circumstances, the extremities of B will be oppositely excited, so as to have a reaction with any similarly excited body, analogous to that which takes place between magnets; since the similarly excited extremities of two such bodies would repel each other, while those dissimilarly excited would be reciprocally attractive. Hence, no doubt, the word polarity is conceived by you to convey an idea of the state of the body B. If I may be allowed

to propose an epithet to convey the idea which I have of the state of a body thus electrified, I would designate it as an electro-polar state, or as a state of electro-polarity.

43. It does not appear to me, that in the suggestion of the electro-polarity, which we both conceive to be induced upon the body B (4), so long as it concerns a mass of ponderable matter, there is any novelty. The only part of your doctrine which is new, is that which suggests an analogous state to be caused in the particles of the bodies through which the inductive power is propagated. Admitting each of the particles of a dielectric, through which the process of ordinary induction takes place, to be put into the state of the body B, it does not appear to me to justify your definition of electrical induction. I think that, consistently with your own exemplification of that process, you should have alleged ordinary induction to be *productive* of an *affection* of particles, *causing* in them a species of polarity. In the case of the bodies A, B, C (4), B is evidently passive. How, then, can we consider as active, particles represented to be in an analogous state? If in B there is no action, how can there be any action in particles performing a perfectly similar part? Moreover, how can the inductive power of an electrical accumulation upon A consist of the polarity which it induces in B?

44. Having supposed (8) an electrified ball, A, an inch in diameter, to be situated within a thin metallic sphere, C, of a foot in diameter, you suggest, that where one thousand concentric metallic spheres interpose between A and the inner surface of C, the electro-polar state of each particle in those spheres would be analogous to that of B, already mentioned. Of course, if there be any action of those particles, there must be an action of B; but this appears to me not only irreconcilable with any previously existing theory, but also with your own exposition of the process by which B is polarized.

45. Supposing concentric metallic hemispheres to be interposed only upon one side of A, you aver that, agreeably to your experience, more of the inductive influence would be extended towards that side of the containing shell than before (14.) Admitting this, I cannot concede that the greater influence of the induction, resulting from the presence of the metallic particles, is the consequence of any *action* of theirs, whether in *contiguity* or in *proximity*. Agreeably to my view, the action is confined to the electrical accumulation in the sphere A. Between the electricity accumulated in this sphere, and that existing in or about the intervening ponderable particles, there may be a *reaction*; but evidently these particles are as inactive as are the steps of a ladder in the scaling of a wall.

46. Suppose a powerful magnet to be so curved as to have the terminating polar surfaces parallel, and to leave between them an interval of some inches. Place between these surfaces a number of short pieces of soft iron wire. These would, of course, be mag-

netized, and would arrange themselves in rows, the north and south poles becoming contiguous. Would this be a sufficient reason for saying that the inductive influence of the magnetic poles was an *action* of the contiguous wires? Would not the phenomena be the consequence of an *affection* of the contiguous pieces of wire, not of their *action*?

47. As respects the word *charge*, I am not aware that I have been in the habit of attaching any erroneous meaning to it, as your efforts to define it in paragraph 3 would imply. I have been accustomed to restrict the use of it to the case which you distinguish as an inductive charge, illustrated by that of the Leyden jar. To designate the states of the conductors of a machine, I have almost always employed the words *excited* or *excitement*. In my text book these words are used to designate the state of glass or resin electrified by friction, while that of coated surfaces, whether panes or jars, inductively electrified, has been designated by the words *charge* or *charged*.

48. I understood the word *contiguous* to imply contact, or contiguity, whereas it seems that it was intended by you to convey the idea of proximity. In the last-mentioned sense it is not inconsistent with the idea of an action, at the distance of half an inch; but by admitting the word *contiguous* to be ill chosen, you have with great candour furnished me with an apology for having mistaken your meaning.

49. Any inductive action which does not exist at sensible distances (20), you attribute to *ordinary* induction, considering the case of induction through a vacuum as an *extraordinary* case of induction. To me it appears that the induction must be the same in both cases, and that the *circumstances* under which it acts are those which may be considered in the one case as *ordinary*, in the other *extraordinary*. Thus take the case cited in your reply (8, 9, 10). Does the interposition of the spheres alter the character of the inductive power in the sphere A?

50. Either the force exercised by the charge in A is like that of gravitation, altogether independent of the influence of intervening bodies, or, like that of light, is dependent on the agency of an intervening matter. Agreeably to one doctrine, the matter, by means of which luminous bodies act, operates by its transmission from the luminous surface to that illumined; agreeably to another doctrine, the illuminating matter operates by its undulations. If the inductive *power* of electrified bodies be not analogous to gravitation, it must be analogous to the power by which light is produced, so far as to be dependent on intervening matter. But were it to resemble gravitation, like that force it would be uninfluenced by intervening matter. If your experiments prove that electrical induction is liable to be modified by intervening matter, it is demonstrated that in its mode of operating it is analogous to light, not to gravitation. It is then proved that, agreeably to your doc-



trine, electrical induction requires the intervention of matter; but you admit that it acts across a vacuum, and, of course, acts without the presence of *ponderable* matter. Yet it requires intervening matter of some kind, and since that matter is not ponderable, it must of necessity be imponderable. When light is communicated from a luminous body in the centre of an exhausted sphere, agreeably to the undulatory hypothesis, its efficacy is dependent on the waves excited in an intervening imponderable medium. Agreeably to your electro-polar hypothesis, the inductive efficacy of an electrified body in an exhausted sphere would be due to a derangement of electric equilibrium, by which an opposite electric state would be produced at the surface of the containing sphere from that at the centre (26, 27). This case you consider as one of extraordinary induction; but when air is admitted into the hollow sphere, or when concentric spheres are interposed, you hold it to be a case of ordinary induction. Let us then, in the case of the luminous body, imagine that concentric spheres of glass are interposed, of which the surfaces are roughened by grinding. In consequence of the roughness thus produced, the rays, instead of proceeding in radii from the central ball, would be so refracted as to cross each other. Of the two instances of illumination, thus imagined, would the one be described as *ordinary*, the other as *extraordinary radiation*? But if these epithets are not to be applied to radiation, wherefore, under analogous circumstances, are they applicable to induction? Wherefore is induction, when acting through a plenum, to be called ordinary, and yet, when acting through a vacuum, to be called extraordinary? In the well-known case of the refracting power of Iceland spar, light undergoes an *ordinary* and *extraordinary refraction*; not an *ordinary* and *extraordinary radiation*. The candle, of which, when viewed through the spar, two images are seen, does not *radiate ordinarily* and *extraordinarily*.

51. If there be occasionally, as you allege (21), large intervals between the particles of radiant heat, how can the distances between them resemble those existing between particles acting at distances which are not sensible? The repulsive reaction between the particles of radiant caloric, as described by you (21), resembles that which I have supposed to exist between those of electricity; but I cannot conceive of any description less suitable for either, than that of particles which do not act at sensible distances.

52. Aware that the materiality of heat, and the Newtonian theory, which ascribes radiation to the projection of heat or light-producing particles, have been questioned, I should not have appealed to a doctrine which assumes both the materiality of heat and the truth of the Newtonian theory, had not you led the way; but, agreeably to the doctrine and theory alluded to, I cannot accord with you in perceiving any similitude between the processes of conduction and radiation (21).

53. Consistently with the hypothesis that electricity is material,

you have shown that an enormous quantity of it must exist in metals. To me it seems equally evident that, agreeably to the idea that heat is material, there must exist in metals a proportionably great quantity of caloric. The intense heat produced when wires are deflagrated by an electrical discharge, cannot otherwise be consistently accounted for. Agreeably to the same idea, every metallic particle in any metallic mass must be surrounded by an atmosphere of caloric ; since the *changes* of dimensions, consequent to variations of temperature, can only be explained by corresponding variations in the quantity of caloric imbibed, and in the consequent density of the calorific atmospheres existing in the mass which undergoes these changes\*.

54. Such being the constitution of expansible bodies agreeably to the hypothesis in question, it seems to me that the process by which caloric is propagated through them by *conduction*, must be extremely different from that by which it is transmitted from one part of space to another by *radiation*. In the one case, the calorific particle flies like a bullet projected from a gun, but with an inconceivably greater velocity, which is not sensibly retarded by the reflecting or refracting influence of intervening transparent media ; in the other case, it must be slowly imparted from one calorific atmosphere to another, until the repulsion, sustained on all sides, is in equilibrio. It is in this way that I have always explained the fact that metals are bad radiators, while good reflectors.†

55. In paragraph 25 you allege that conduction of heat differs from electrical induction, because it passes by a very slow process ;

\* I subjoin the language which I have held respecting the constitution of expansible solids, during the last twenty years.

"The expansion of matter, whether solid, liquid, or æriform, by an increase of temperature, may be thus explained :—

"In proportion as the temperature within any space is raised, there will be more caloric in the vicinity of the particles of any mass contained in the space. The more caloric in the vicinity of the particles, the more of it will combine with them ; and in proportion to the quantity of caloric thus combined, will they be actuated by that reciprocally repellent power, which, in proportion to its intensity, regulates their distance from each other.

"There may be some analogy between the mode in which each ponderable atom is surrounded by the caloric which it attracts, and that in which the earth is surrounded by the atmosphere ; and as in the latter case, so probably in the former, the density is inversely as the square of the distance.

"At a height at which the atmospheric pressure does not exceed a grain to the square inch, suppose it to be doubled, and supported at that increased pressure by a supply of air from some remote region, is it not evident that a condensation would ensue in all the inferior strata of the atmosphere, until the pressure would be doubled throughout, so as to become at the terrestrial surface 30 pounds instead of the present pressure of 15 pounds ? Yet the pressure at the point from which the change would be propagated would not exceed two grains per square inch.

"In like manner, it may be presumed that the atmospheres of caloric are increased in quantity and density about their respective atoms, by a slight increase in the calorific tension of the external medium."

† I will here quote the rationale which has been given in my lectures for the last twenty years :—

"Metals appear to consist of particles so united with each other, or with caloric, as to leave no pores through which radiant caloric can be projected. Hence the

while induction is, in its distant influence, simultaneous with its force at the place of action. How then can the passage of heat by conduction be "*a process precisely like that of radiation*" (21), which resembles induction in the velocity with which its influence reaches objects, however remote?

56. Although (21) you appeal to the "modern views respecting radiation and conduction of heat," in order to illustrate your conception of the contiguity of the particles of bodies subjected to induction, yet (in 25) you object to the reference which I had made to the same views, in order to shew that the intensity of electro-polarization could not be inversely as the number of the polarized particles, interposed between the "inductric" surfaces. Let us then resort to the case above suggested, of the influence of the surfaces of the poles of a magnet upon intervening pieces of iron wire. In 1679, 14th series, you suggest this as an analogous case to that of the process of *ordinary* electrical induction, which we have under consideration. Should there be, in the one case, a thousand pieces of wire interposed, in the second, a hundred, will it be pretended that the intensity of their reciprocal inductive reaction would be inversely as the number; so that the effect of the last-mentioned number of wires would be *equivalent* to that of the first? Were intervals to be created between the wires, by removing from among the number first mentioned alternate wires, it would seem to me that the energy of their reciprocal influence would be diminished, not only as the number of them might be lessened, but also as they should consequently be rendered more remote.

57. If, as you suggest, the interposition of ponderable particles have any tendency to promote inductive influence (14), there must be some number of such particles by which this effect will be best attained. That number being interposed, I cannot imagine how the intensity of any electro-polarity, thus created in the intervening

only portion of any metallic mass which can yield up its rays by radiation, is the external stratum.

"On the other hand, from its porosity, and probably from its not retaining caloric within its pores tenaciously as an ingredient in its composition, charcoal opposes but little obstruction to the passage of that subtle principle, when in the radiant form; and hence its particles may all be simultaneously engaged in radiating any excess of this principle with which a feeble affinity may have caused them to be transiently united, or in receiving the rays emitted by any heated body, to the emanations from which they may have been exposed.

"We may account in like manner for the great radiating power of earthenware and wood.

"For the same reason that calorific rays cannot be projected from the interior of a metal, they cannot enter it when projected against it from without. On the contrary, they are repelled with such force as to be reflected without any perceptible diminution of velocity. Hence the pre-eminence of metallic reflectors.

"It would seem as if the calorific particles which are condensed between those of the metal, repel any other particles of their own nature which may radiate towards the metallic superficies, before actual contact ensues; otherwise, on account of mechanical imperfection, easily discernible with the aid of a microscope, mirrors would not be as efficacious as they are found to be in concentrating radiant heat. Their influence, in this respect, seems to result from the excellence of their general contour, and is not proportionably impaired by blemishes."

particles, can, by a diminution of their number, acquire a proportionable increase; and evidently in no case can the excitement in the particles exceed that of the "inductric" surfaces, whence the derangement of electrical equilibrium arises.

58. The repulsive power of electricity being admitted to be inversely as the squares of the distances, you correctly infer that the aggregate influence of an electrified ball B, situated at the centre of a hollow sphere C, will be a constant quantity, whatever may be the diameter of C. This is perfectly analogous to the illuminating influence of a luminous body situated at the centre of a hollow sphere, which would of course receive the whole of the light emitted, whatever might be its diameter; provided that nothing should be interposed to intercept any portion of the rays. But in order to answer the objection which I have advanced, that the diminution of the density of a "*dielectric*" cannot be compensated by any consequent increase of inductive intensity, it must be shown, in the case of several similar hollow spheres, in which various numbers of electrified equidistant balls should exist, that the influence of such balls upon each other, and upon the surfaces of the spheres, would not be directly as the number of the balls, and inversely as the size of the containing spaces. Were gas-lights substituted for the balls, it must be evident that the intensity of the light in any one of the spheres would be as the number of lights which it might contain: now one of your illustrations (8), above noticed, makes light and electrical induction obey the same law as respects the influence of distance upon the respective intensities.

59. From these considerations, and others above stated, I infer, that if electrical induction were an action of particles in proximity, operating reciprocally with forces varying in intensity with the squares of the distances, their aggregate influence upon any surfaces, between which they might be situated, would be proportionable to their number; and since experience demonstrates that the inductive power is not diminished by the reduction of the number of the intervening particles, I conclude that it is independent of any energy of theirs, and proceeds altogether from that electrical accumulation with which the inductive change is admitted to originate.

60. In paragraph 31, you say, "that at one time there was a distinction between heat and cold. At present that theory is done away with, and the phænomena of heat and cold are referred to the same class, and to different degrees of the same power."

61. In reply to this, I beg leave to point out, that although, in ordinary acceptance, cold refers to relatively low temperature, yet we all understand that there might be that perfect negation of heat, or abstraction of caloric, which may be defined absolute cold. I presume that, having thus defined absolute cold, you would not represent it as identical with caloric. For my own part, this would seem as unreasonable as to confound matter with nihility.

62. Assuming that there is only one electric fluid, there appears

to me to be so far an analogy between caloric and electricity, that negative electricity conveys, in the one case, an idea analogous to that which cold conveys in the other. But if the doctrine of Du Fay be admitted, there are two kinds of electric matter, which are no more to be confounded than an acid and an alkali. Let us, upon these premises, subject to further examination your argument (1330), that insulation and conduction should be identified, "*since the moment we leave in the smallest degree perfection at either extremity, we involve the element of perfection at the opposite end.*" Let us suppose two remote portions of space, one replete with pure vitreous electricity, the other with pure resinous. Let there be a series of like spaces, containing the resinous and vitreous electricities in as many different varieties of admixture, so that in passing from one of the first-mentioned spaces, through the series to the other, as soon as we should cease to be exposed to the vitreous fluid, in perfect purity, we should begin to be exposed minutely to the resinous; or that, in passing from the purely resinous atmosphere, we should begin to be exposed to a minute portion of the vitreous fluid; would this be a sufficient reason for confounding the two fluids, and treating the phenomena to which they give rise as the effect of one only?

63. But the discussion into which your illustrations have led me refers to things, whereas conduction and insulation, as I understand them, are opposite and incompatible properties; so that, inasmuch as either prevails, the other must be counteracted. Conduction conveys to my mind the idea of *permeability* to the electric fluid, insulation that of *impermeability*; and I am unable to understand how these irreconcilable properties can be produced by a difference of degree in any one property of electrics and conductors.

64. If, as you infer, glass have, comparatively with metals, an almost infinitely minute degree of the conducting power, is it this power which enables it to prevent conduction, or, in other words, to insulate? Let it be granted that you have correctly supposed conduction to comprise both induction and discharge, the one following the other in perfect conductors within an inexpressibly brief interval: insulation does not prevent induction, but, so far as it goes, prevents discharge. In practice, this part of the process of conduction does not take place through glass during any time ordinarily allotted to our experiments, however correct you may have been in supposing it to have ensued before the expiration of a year or more, in the case of the tubes which you had sealed after charging them. But conceding it to have been thus proved, that glass has, comparatively with metals, an infinitely small degree of the conducting power, is it this minute degree of conducting power which enables it to prevent conduction, or, in other words, to insulate?

65. Induction arises from one or more properties of electricity, insulation from a property of ponderable matter; and although there be no matter capable of preventing induction, as well as discharge,

were there such a matter, would that annihilate insulation? On the contrary, would it not exhibit the property in the highest perfection?

66. As respects the residual charge of a battery, is it not evident that any electrical change, which affects the surface of the glass, must produce a corresponding effect upon the stratum of air in contact with the coating of the glass? If we place one coating between two panes, will it not enable us, to a certain extent, to charge or discharge both? Substituting the air for one of them, will it not in some measure be liable to an affection similar to that of the vitreous surface, for which it is substituted? In the well-known process of the condensing electrometer, the plate of air interposed between the discs is, I believe, universally admitted to perform the part of an electric, and to be equivalent in its properties to the glass in a coated pane.

67. When I adverted to a gradual relinquishment of electricity by the air to the glass, I did not mean to suggest that it was attended by any more delay than the case actually demonstrates. It might be slow or gradual, compared with the velocity of an electric discharge, and yet be extremely quick comparatively with any velocity ever produced in ponderable matter. That the return should be slow when no coating was employed, and yet quick when it was employed, as stated by you (38), is precisely what I should have expected, because the coating only operates to remove all obstruction to the electric equilibrium. The quantity or intensity of the excitement is dependent altogether upon the electrified surfaces of the air and the glass. You have cited (1632) the property of a charged Leyden jar, as usually accounted, of electrifying a carrier ball. This, I think, sanctions the existence of a power to electrify, by convection, the surrounding air to a greater or less depth; since it must be evident that every aerial particle must be competent to perform the part of the carrier ball.

68. Agreeably to the Franklinian doctrine, the electricity directly accumulated upon one side of a pane repels that upon the other side. You admit that this would take place were a vacuum to intervene; but when ponderable matter is interposed, you conceive each particle to act as does the body B, when situated as described between A and C (4). But agreeably to the view which I have taken, and what I understand to be your own exposition of the case, B is altogether passive, so that it cannot help, if it does not impede, the repulsive influence. Moreover, it must be quite evident, that were B removed, and A approximated to C, without attaining the striking distance, the effect upon C, and the consequent energy of any discharge upon it from A, would be greater instead of less. If, in the charge of a coated pane, the intermediate ponderable vitreous particles have any tendency to enhance the charge, how happens it that, the power of the machine employed being the same, the intensity of the charge which can be given to an electric is greater in proportion to its tenuity?

69. In reference to the direction of any discharge, it appears to me, that as, in *charging*, the fluid must always pass from the cathode to the anode, so in reversing the process it must pursue the opposite course of going from the anode back to the cathode. Evidently the circumvolutions of the circuit are as unimportant as respects a correct idea of the direction, as their length has been shown by Wheatstone to be incompetent to produce any perceptible delay.

70. The dissipation of conductors being one of the most prominent among electrical phænomena, it appears to me to be an objection to your theory, if, while it fails to suggest any process by which this phænomenon is produced, it assumes premises which seem to be incompatible with the generation of any explosive power. If discharge only involves the restoration of polarized ponderable particles to their natural state, the potency of the discharge must be proportionable to the intensity of the antecedent polarity; yet it is, through conductors, liable, as you allege, to polarization of comparatively low intensity (31) that discharge takes place with the highest degree of explosive violence.

71. Having inquired how your allegation could be true, that discharge brings bodies to their natural state, and yet causes conductors to be dissipated, you reply (34) that different effects may result from the same cause, acting with different degrees of intensity, as when by one degree of heat ice is converted into water, by another into steam. But it may be urged, that although, in the case thus cited, different effects are produced, yet that the one is not inconsistent with the other, as were those ascribed to electrical discharges. It is quite consistent that the protoxide of hydrogen, which, *per se*, constitutes the solid called ice, should, by one degree of calorific repulsion, have the cohesion of its particles so far counteracted as to be productive of fusion; and yet that a higher degree of the same power should cause them to recede from each other, so as to exist in the aëriform state.

72. In order to found, upon the influence of various temperatures, a good objection to my argument, it should be shewn, that while a certain reduction of temperature enables aqueous particles to indulge their innate propensity to consolidation, a still further reduction will cause them, in direct opposition to that propensity, to repel each other so as to form steam.

73. In your concluding paragraph you allege, "*that when ponderable particles intervene, during the process of dynamic induction, the currents resulting from this source do require these particles.*" I presume this allegation is to be explained by the conjecture made by you (1729), that since certain bodies, when interposed, did not interfere with dynamic induction, therefore they might be inferred to co-operate in the transmission of that species of inductive influence. But if the induction takes place without the ponderable matter, is it right to assume that this matter *aids*, because it does not prevent the effect? Might it not be as reasonably inferred, in

the case of light, that, although its transmission does not require the interposition of a pane of glass, yet, that when such a pane is interposed, since the light is not intercepted, there is reason to suppose an active co-operation of the vitreous particles in aid of the radiation? It may be expedient here to advert to the fact, that Prof. Henry has found a metallic plate to interfere with the dynamic induction of one flat helix upon another. I have myself been witness of this result.

74. Does not magnetic or electro-dynamic induction take place as well *in vacuo* as *in pleno*? Has the presence of any gas been found to promote or retard that species of reaction? It appears that, agreeably to your experiments, ponderable bodies, when made to intervene, did not enhance the influence in question, while in some of those performed by Henry it was intercepted by them. Does it not follow that ponderable particles may impede, but cannot assist in this process?

75. I am happy to find that, among the opinions which I expressed in my letter to you, although there are several in which you do not concur, there are some which you esteem of importance, though you do not consider yourself justified in extending to them your sanction, being constrained, in the present state of human knowledge, to hold your judgment in suspense. For the present I shall here take leave of this subject, having already so extended my letter as to occupy too much of your valuable time. I am aware that as yet I have not sufficiently studied many of the intricate results of your sagacity, ingenuity, and consummate skill in experimental investigations. When I shall have time to make them the subject of the careful consideration which they merit, I may venture to subject your patience to some further trials.

Philadelphia, Jan. 1, 1841.

*On the Combinations of the Volatile Chlorides with Ammonia, and on their State of Composition; (Zusammensetzungsweise.)* By HEINRICH ROSE.

(Translated from Poggendorff's *Annalen der Physik und Chemie.* B. lii. s. 1.)

Some considerable time has now elapsed since I compared the compounds of the oxysalts and ammonia with the compounds which are formed by these salts and water. I have shown, also, that the compounds of ammonia with the unvolatile metallic chlorides, which, in most of their properties, are so similar to the oxysalts, might be considered analogous to the compounds of the same salts with water.\*

This view, at first but little attended to, subsequently became ge-

\* Poggendorff's *Annalen.* Bd. xx. s. 147.



nerally adopted, especially after Berzelius had called attention to the distinction between the compounds of *ammonia*, and those of the *oxide of ammonium*.

I afterwards examined the compounds of volatile chlorides with ammonia, and sought means to compare them with those which they produce with phosphuretted hydrogen.\*

In their *properties*, however, only some of these combinations are analogous to each other. It is principally the chloride of titanium, the chloride of tin, and the chloride of aluminium, which, with phosphuretted hydrogen, give compounds that can be compared with those of ammonia. Chloride of iron, liquid chloride of phosphorus, and chloride of sulphur, all of which form compounds with ammonia, form none with phosphuretted hydrogen; but, on the contrary, these substances mutually decompose one another.

Perhaps the compounds of the chlorides with ammonia may be more accurately compared with those which they form with water, in the same manner as the ammoniacal unvolatile metallic chlorides. The hydrates of the volatile chlorides are not, however, sufficiently known, but they have, with the analogous ammoniacal compounds, these properties in common, viz., that from neither of them can the water be separated by heat, and that the volatile chlorides cannot easily be exhibited from them.

The volatile chloro-compounds take up various quantities of ammonia; but there is no law by which we can predict, *a priori*, the quantity of ammonia that the volatile chlorides will take up, any more than we can foretell the quantity of water of crystallization, or the quantity of ammonia in its combinations with oxysalts, or with unvolatile metallic chlorides. But by comparing these, one with another, it seems as if a somewhat empirical law was followed, and that the chlorides, whose radical with oxygen forms a stronger acid, take up more ammonia than those whose radical with oxygen forms so feeble an acid, that, in combination with ammonia, they produce no saltlike substance.

A comparison of the composition of the compounds of the volatile chlorides with ammonia, which have been examined, will shew the probability of the above assertion. I shall separate these combinations into two classes. The first class shall comprise those compounds whose chlorides correspond to feeble acids; and the second class those whose chlorides correspond to the stronger acids.

#### FIRST CLASS.

1. Ammoniated chloride of titanium:  $\text{Ti Cl}^3 + 2 \text{ N H}^3$  according to my examination;† but  $\text{Ti Cl}^3 + 3 \text{ N H}^3$  according to Persoz.‡

\* Poggendorff's Annalen. Bd. xxiv. s. 109.

† Poggendorff's Annalen. Bd. xxiv. s. 145.

‡ Annales de Chimie et de Physique, t. xlv. p. 321. Persoz states that the combination contains in one hundred 65·861 of chloride of titanium, and 34·139 of ammonia. A compound according to the above given formula, contains in one hundred 64·89 chloride of titanium, and 35·11 of ammonia.

2. Ammoniated chloride of tin :  $\text{Sn Cl}^2 + \text{N H}^3$  according to my examination ;\* but  $\text{Sn Cl}^2 + 2 \text{N H}^3$  according to Persoz.

3. Ammoniated chloride of aluminium :  $\text{Al Cl}^3 + 3 \text{N H}^3$  according to my examination, which agrees with Persoz.†

4. Ammoniated chloride of iron :  $\text{Fe Cl}^3 + \text{N H}^3$  according to my examination.‡

5. Ammoniated chloride of sulphur :  $\text{S Cl} + \text{N H}^3$  according to my examination.§

6. Ammoniated chloride of antimony :  $\text{Sb Cl}^3 + 2 \text{N H}^3$  according to my examination.||

Only in the compounds of ammonia with chloride of titanium, chloride of aluminium, and the chloride of sulphur, does the quantity of ammonia precisely answer to form chloride of ammonium with the chlorine of the chloride, when the chloride takes up the elements of the water: the other chlorides take up less ammonia. We must not, however, as will presently appear, consider these compounds, even after treatment with water, as compounds of chloride of ammonium with oxides; but as ammoniacal compounds of a peculiar kind, comparable with, and similar to the combinations of many anhydrous acids—especially sulphuric acid with ammonia (sulphat-ammon).¶ This conclusion immediately becomes probable, from the solubility of most of these compounds in water, even when the oxide which they contain is, of itself, insoluble either in water or in solutions of ammoniacal salts; and still more so, from the circumstance that in the solutions of these compounds, the ammonia can only be *partially*, and never *completely* separated by a solution of chloride of platinum; which is also the case with solutions of sulphat-ammon and parasulphat-ammon.

#### SECOND CLASS.

To this class belong but a few compounds; for it is those radicals, which, with oxygen, form the strongest acids, that give no corresponding compounds with chlorine; at least, we know of no chlorides which correspond with sulphuric acid, chromic acid, nitric acid, molybdic acid, tungstic acid, or arsenic acid. Many chlorides, which correspond with pretty strong acids, appear to have no tendency to combine with ammonia. It is almost solely the liquid compounds of chlorine with phosphorus and with arsenic, which, in constitution, are analogous to the phosphorous and arsenious acids, that here need be mentioned.

*Ammonia-protochloride of Phosphorus.* \*\* — According to my analysis, this compound agrees with the formula  $\text{P Cl}^3 + 5 \text{N H}^3$ ,

\* Poggendorff's Annalen. Bd xxiv. s. 163.

† Ibid. s. 298. ‡ Ibid. s. 301. § Ibid. s. 306.

|| Ibid. Bd. xx. s. 160.

¶ Annals of Electricity. Vol. vi. p. 433.

\*\* The original term is "phosphorchlorür-ammoniak," which might be translated "protochloride of phosphorus-ammonia"—certainly neither an elegant nor an intelligent phrase. The translator is not certain, however, that he has coined the most suitable expression.

and contains just so much ammonia, that when the compound is dissolved in water, a neutral phosphite of the oxide of ammonium, and a chloride of ammonium might be produced.\* Persoz, notwithstanding his statement, that the compound, by treatment with water, would be converted into chloride of ammonium and neutral phosphite of ammonia, has found in it only four atoms of ammonia ( $\text{P Cl}^3 + 4 \text{N H}^3$ ).† The liquid chloride of phosphorus, even when considerably cool, absorbs gaseous ammonia with avidity. I have paid most particular attention to the circumstance, that the compound can be obtained pure only when the chloride is slowly, and with great depression of temperature, brought into contact with the ammoniacal gas.

I have already, some time ago, taken much fruitless trouble to obtain a combination of ammonia with the solid chloride of phosphorus, in definite proportions, so as to be in accordance with phosphoric acid;‡ and I have more recently repeated those experiments, with the same unfavourable results. I will briefly communicate the results of these latter experiments:—

The solid chloride of phosphorus becomes heated with the greatest violence when treated with dried ammoniacal gas. If, when the mass has become strongly heated by complete saturation with ammonia, it be treated with water, phosphuretted nitrogen remains undissolved, and in the solution no phosphoric acid can be detected by reagents: it contains chloride of ammonium only. If, on the contrary, the solid chloride of phosphorus be treated with dried ammoniacal gas, slowly and by considerable cooling, it absorbs almost none of it: it can be kept for a long time without change, in a flask charged with ammoniacal gas and perfectly closed. When it is dissolved in water, this happens with evolution of great heat, as is the case with pure chloride of phosphorus; and in the solution some flocks of undissolved phosphuretted nitrogen are seen floating. The liquid contains only phosphoric acid and hydrochloric acid. These observations agree, in some part, with those of Liebig and Whöler, published some time ago.§

*Ammoniated Protochloride of Arsenic.*||—I have not previously prepared this compound. Persoz states that it may be composed according to the formula  $\text{As Cl}^3 + 2 \text{N H}^3$ .¶

For the experiments that I more recently made to ascertain the composition of this compound, I employed a protochloride of arsenic, which had been partly obtained by distillation of a mixture of powdered arsenious acid, chloride of sodium, and sulphuric acid; and partly by treating metallic arsenic with chlorine gas. The proto-

\* Poggendorff's Annalen. Bd. xxiv. s. 308, and Bd. xxviii. s. 529.

† Annales de Chimie et de Physique, t. xlv. p. 320.

‡ Poggendorff's Annalen. Bd. xxiv. s. 311.

§ Annalen der Pharmacie. Bd. xi. s. 139.

|| Arsenikchlorür-Ammoniak.

¶ Annales de Chimie et de Physique. t. xlv. p. 320

chloride obtained by the latter method was carefully freed from adhering chlorine.

If the protochloride be treated with dry ammoniacal gas, we obtain, during the heat, a white powder, which is perfectly soluble in water. 1·862 grammes of the well saturated compound, dissolved in water, to which were added some nitric acid, and afterwards nitrate of silver, gave 3·323 grammes of chloride of silver.

1·368 grammes of the combination, from another preparation, gave, by similar treatment, 2·4195 grammes of chloride of silver. If we reckon the composition of *ammoniated protochloride of arsenic* as one hundred, these experiments give the following results :—

	I.	II.
Chlorine.....	44·02 ..	43·63
Arsenic.....	31·16 ..	30·89
Ammonia.....	24·82 ..	25·48
	<hr/>	<hr/>
	100·00	100·00

The second quantity of the examined compound retained some adhering ammonia; for it had been a long time confined in the vessel with dry ammoniacal gas. The solution of this combination reacted also stronger alkaline than that of any other portion of the compound.

The composition, thus found, agrees with the formula  $\text{As Cl}^3 + 7 \text{ N H}^3$ ; or rather with  $2 \text{ As Cl}^3 + 7 \text{ N H}^3$ . The composition, calculated according to this formula, contains, in 100 :—

Chlorine.....	44·00
Arsenic.....	31·14
Ammonia.....	24·86
	<hr/>
	100·00

By its solution in water there might be formed, supposing all the chlorine of the chloride of arsenic converted into chloride of ammonium, an acid arsenite of the oxide of ammonium,  $2 \ddot{\text{As}} + \text{N H}^4$  (the neutral salt is most probably  $\ddot{\text{As}} + \text{N H}^4$ ).

I have in vain attempted to prepare a chloride of arsenic corresponding to arsenic acid, in order to examine a compound of it with ammonia. Liebig and Wöhler have already failed to prepare it.\* I have obtained the same results as they, by attempting to distil a mixture of arsenic acid and chloride of sodium with sulphuric acid. I obtained, under great effervescence, chlorine and protochloride of arsenic. Even when metallic arsenic is treated with much excess of chlorine gas, and the obtained protochloride of arsenic kept long in contact with chlorine, no perchloride of arsenic can be obtained.

I attempted to combine *chloride of selenium* ( $\text{Se Cl}^4$ ) with ammonia; but the chloride takes up none of it in the cold, and, by

\* Annalen der Pharmacie. Bd. xi. s. 150.

heating it decomposes. It appears generally, that the liquid chlorides only, and not the solid ones, combine energetically with ammonia.

From the few compounds which ammonia forms with the chlorides, in accordance with the more powerful acids, we distinctly see that they contain more ammonia than the combinations of ammonia with those chlorides which form such feeble acids that they are unable, with ammonia, to form saltlike compounds. The latter, at the utmost, take up only so much ammonia as, on the solution of the compound in water, is sufficient, with the produced hydrochloric acid, to form the chloride of ammonium; frequently, indeed, they take up even much less ammonia. The other chlorides, on the contrary, take up more ammonia; and, indeed, so much, that they can form a neutral ammoniacal salt with the acid, and also chloride of ammonium.

The phosphorous acid is obviously a more powerful acid than the arsenious acid. By the dissolution of *ammoniated protochloride of phosphorus* in water, a neutral phosphite of the oxide of ammonium and chloride of ammonium are consequently formed; whereas, by treating the *ammoniated protochloride of arsenic* with water, besides chloride of ammonium, the binarsenite of the oxide of ammonium is produced.

We may, therefore, with probability, conclude, that if the chlorides of sulphur, or selenium, and of arsenic, which correspond with the sulphuric, selenic, and arsenic acids, were known to us in an insulated state, their combinations with ammonia, if such could be produced, would, by treatment with water, give sulphate, selenate, and arseniate of the oxide of ammonium, as well as chloride of ammonium. Moreover, the compounds of the protochloride of phosphorus, and the protochloride of arsenic, with ammonia, after treatment with water, are not to be considered as mixtures of the chloride of ammonium with salts of the oxide of ammonium, but as peculiar ammoniacal compounds, similar to the ammons. Their full amount of ammonia cannot, by any known means, be precipitated from their solutions in water; but only in part, by chloride of platinum.

(To be completed in the next number.)

*Question of Priority relative to the Formation of Photographic Images on Paper, which, being little or none visible on being removed from the dark chamber, subsequently become visible.* Letter from M. BAYARD.\*

In the last session of the Academy, M. Biot read a letter from M. Talbot, in which that philosopher speaks of a means, which he has

\* Comptes Rendus.

not made known, of rendering visible a photographic impression which is invisible when the paper is taken from the darkened chamber. It is now a long time since I discovered three different processes, which conduce to this result. Permit me, Monsieur, to make one of them known to you, and when time will permit me to repeat the two others, I shall have the honour to communicate them to you. A paper having been prepared with the bromide of potassium, then with the nitrate of silver, is exposed, whilst still in a humid state, and during a period of some minutes, in the focus of the darkened chamber. On this paper being withdrawn, and examined with the light of a taper, no trace whatever of an image can be discovered; still, however, it is imprinted thereon. In order to render it apparent, it will be sufficient to expose the paper to the vapour of mercury, as is done with the plates in the process of M. Daguerre. It immediately becomes coloured black in every place where the light has modified the preparation. It is almost unnecessary to observe that care must be taken to avoid as much as possible leaving any impression on the prepared paper from any other luminous radiation than that from the darkened chamber.

The above description, and one or two proofs obtained by this process, have been addressed to the Academy, which, in the session of 11th November, 1839, willingly accepted the deposit. I shall feel obliged, Monsieur, if you deem it *apropos*, to lay the contents of the accompanying packet before the Academy.

The sealed packet deposited by M. Bayard having been opened, was found to contain a photographic proof on paper, accompanied by the following note. Another proof was also appended to the letter:—

#### PHOTOGRAPHIC PROCESS ON PAPER.

The accompanying photographic image was obtained on the 24th October, 1839, in eighteen minutes, namely, from eleven o'clock in the morning to eighteen minutes past eleven, by the following process:

Soak the paper in a feeble solution of chloride of sodium; when it is well dried, pass over this paper a solution consisting of nitrate of silver, dissolved in six times its weight in water.

The paper being almost dry, and secured from all action of the light, is now to be exposed to the vapour of sodium, then in the darkened chamber, then to mercury, as in the process of M. Daguerre, and finally, is to be washed in a solution of the hyposulphate of soda.

When the paper is withdrawn from the darkened chamber, scarcely any traces of the design are perceptible; but immediately after the mercurial vapour condenses on the paper, the images make their appearance, as on the metallic plates, with this difference, that the images are produced in an inverted form, as in the process of M. Talbot.

Paris, November 8th, 1839.

*Remarks of M. BIOT on the preceding letter.*

There appears to be this essential difference between the process described by M. Bayard and that announced by M. Talbot, that M. Bayard presents as a condition, that the paper which has received his preparation be exposed, whilst still wet, to the radiation in the darkened chamber; whilst M. Talbot says that his paper preserves its sensibility even after a lapse of several months. In truth, M. Talbot does not say if this maintaining of its sensibility is not rather a restitution which is produced by wetting the paper; neither does M. Bayard any more say whether he could produce in his paper a similar restitution of the primitive sensibility.

*Observations on the Diurnal Variation of the Magnetic Needle.*

By M. DUTROCHET.\*

Two magnetic needles which have an unequal magnetism, and which, having their poles reciprocally reversed, forming together a system imperfectly astatic, suspended by a film of silk, are, one in relation to the other, in such conditions, that the superior needle, which has more magnetic force than the inferior one, undergoes, by the antagonism of this latter, only a simple diminution of its declination, in such a manner that it approaches to about  $15^{\circ}$  of the terrestrial meridian. Now, in observing this superior needle, which corresponds to a graduated circle, I have remarked that it undergoes in its direction, which is that of the system of which it forms a part, diurnal variations, of which the deflections are exactly similar to that which the simple magnetic needle presents, but with far greater amplitudes:—with the simple magnetic needle, in fact, the maximum of the amplitude of the diurnal variation does not attain to 25 minutes, whilst with my system of needles, I have observed, in certain days of the summer, a variation which has attained  $3^{\circ}$  of amplitude in its maximum. I have observed this deflection so considerably amplified from the diurnal variation during a period of more than four months; and also that from what has been observed with the simple magnetic needle, this movement has always been in relation with the state, ascendant or descendant, of the atmospheric temperature, taking place towards the west so much that the temperature of the exterior air crossed and affected its movement towards the east, in every case where the temperature decreased.

M. Arago spoke on the subject of this communication, and observed that the principle which serves as the basis of the experiment of M. Dutrochet was known. It has long been known that a magnetic needle, of which the natural direction is changed by being in the

\* Comptes Rendus. Feb. 8, 1841.

neighbourhood of a magnet, may undergo diurnal variations much more extensive than those which are in the ordinary state. In some experiments these variations have attained an amplitude of 10 degrees.

M. Dutochet replied :—

If the principle of the experiment which I have just been detailing is known, the form of this experiment itself, such as I have presented it, is new. The augmentation of the amplitude of the diurnal variations of the magnetic needle, deviating from its natural direction by the influence of a fixed magnet, has been observed; but it has not been previously observed that this same augmentation of the amplitude of the diurnal variations diminishes its declination by the influence of another magnetic needle, which forms with it one of the same system, and of which the poles are opposed to its own. This latter needle performs here the part which, in the experiment referred to by M. Arago, belongs to the *permanent* magnet, the modifier of the natural direction of the magnetic needle; but in my experiment the magnet is *moveable*, and follows the magnetic needle in all its movements. When the magnet is *fixed*, its influence on the direction of the magnetic needle changes with each variation in the direction of the latter; since, then, this influence takes place under an angle incessantly differing, whilst, when the magnet is *moveable*, and follows the magnetic needle in all its movements, its influence thereon does not change, and there ought to result from this arrangement more regularity in the relations of the magnetism of this needle with the terrestrial magnetism. The apparatus of which I have made use also has constantly presented to me the diurnal variations of the magnetic needle with an exactness which has appeared to me to be truly grand.

*Researches on the Causes of the Electricity of the Clouds.*

By M. PELTIER.\*

Since the application of moveable electrical apparatus to meteorology, in 1752, several philosophers have remarked the inequality of the indication of these instruments. Romas, Muschenbroëk, the Prince of Gallitzin, and especially B. de Saussure, have perceived the electroscopes vary in their deflections, as the instruments may be elevated or lowered. De Saussure, studying these variations with care, recognized therein all the signs of the development of the electricity of influence, and not those of permanent electricity, which gives contact; however, contrary to his own unbiassed observation, he concluded therefrom that the atmosphere was electric. By entirely doing away with this preconceived idea, and confining our-

\* Comptes Rendus.



selves to facts, their co-ordination has conducted us to another interpretation of the electrical phenomena of the atmosphere.

It is well known that electrometers only mark the difference which exists between the electrical tension of the superior limb of the instrument and the middle, where the leaves of gold are immersed. The instrument is brought into equilibrium by touching both the upper and lower parts at once; the leaves are then at Zero. If the shank is surmounted by a polished ball, we may leave the electrometer exposed to the agitation of the air, under a serene sky, for as long a period as we may think proper, or we may remove it from place to place, still preserving it at the same height, without its manifesting the least sign of electricity. If the shank terminates in a point, it will sometimes take a little electricity in rather more than an hour, but this time will be much abridged if we terminate the shank by a bundle of very fine metallic wires, or by an incandescent body, as was performed by Volta. In the first case, where the instrument remained mute, if we raise it a few *decimetres*, the leaves immediately diverge *positively*; if we replace it at the height of its equilibration, the leaves return to Zero; if we lower it to the same number of *decimetres* below this height, the leaves diverge, but then they are negative; if we again raise the instrument, they return anew to Zero. Thus it is sufficient to raise the instrument in order to obtain positive signs, and to lower it to have negative ones. Providing an equilibrium at all heights, we may make it speak at pleasure, positively or negatively, in all the layers of the air. De Saussure sought the explication of this fact in the influence of the electricity of the air, forgetting that the air, in its agitation, would immediately give to the instrument a permanent electricity, and not a transitory electricity. For the remainder, we reproduce in the study the same effects as under a serene sky, by operating under an insulated ball, suspended from the ceiling, and positively electrized. In the same manner we reproduce the free radiation of the electricity of influence, by replacing the polished ball by a bundle of points, or by an inflamed body.

These experiments shew that the earth acts as a powerful negative body, and the space of the heavens as a body powerfully positive, and that all the bodies interposed between them are electrized by influence, and not by the contact of the air. We cannot too much protest against the words *positive* and *negative*, and the errors of which they are the means of being committed each day. Force to employ the terms already in use, we do not attach to them any other idea than that of indicating the different degrees of the same phenomenon.

The fixed apparatus, as the horizontal wires of Beccaria, or the ordinary vertical bars, are not sufficient to reveal this electricity of influence during the time that the sky is serene: they obey only the influence of electrical clouds. Their indications also are dependent on the hygrometric state of the air. As Beccaria, Schubler, Read,

Clarke, de Dublin, &c., have remarked, the humidity of the air facilitating their radiation.

The result of these observations is, that under a serene sky the atmosphere is not electric; that these instruments are electrized by the influence and not by contact with the air. Again, we have already shewn that the vapours are not bearers of electricity at the moment of their production, but when they are smartly separated from the dissolution by lively projections; that those vapours which are produced at low temperatures, having their electricity neutralized previous to their insulation from the liquid, arrive in the atmosphere only in a neutral state. The electricity of clouds does not proceed from this cause, for, in order to reproduce this in nature, we ought to operate under the same circumstances as she does.

We have found that the celestial space is positive; the water on the surface of the globe is consequently in a negative state, and evaporation is produced under this influence. We have placed under a globe, positively electrized; an insulated vase filled with water, either distilled or not; the vapour which arose from its surface was negative, the remainder of the liquid was positive, and the quantity of the vapour increased as the power of the electrical influence increased. Contrary to what has been said, the vapour which rises at the surface of the sun is consequently negative; the instruments also become electrized by influence in a much less degree as the vapours are more dense, and enclose the instrument better, in an atmosphere uniformly electrical; receiving the negative influence from all sides, as in an electrized sphere, there is no difference in its manifestations; it becomes necessary, then, to make this vapour predominate by means of a kite, to find again the positive influence of the superior space.

When the elastic vapours have been condensed in opaque clouds by a cooling down, and when the temperature, afterwards rising, provokes a new evaporation, this latter is done under the influence positive superior, that is to say, that the first vapours produced have their negative tension augmented at the expense of the inferior strata of the cloud, maintained in the positive state by the terrestrial influence; the result of this is, that the first elastic vapours, formed in the second evaporation, are strongly negative, and those last formed become positive, since the terms are only indicative of their relations to each other. When a new depression of the temperature condenses new vapours, the superior masses form negative clouds, and the inferior masses positive clouds. The phenomenon is represented very well by making a cloud of a considerable number of very small balls of soap in an insulated glass capsule, and submitting it to the positive action of a globe. We see the superior balls grow longer, shoot forward, dissolve, and disappear, leaving the rest of the cloud charged with positive electricity; and if we make the cloud negative, their dissolution increases in quickness.

*On a Process by the aid of which the Variations of the Magnetic Needle may be increased.* Note communicated by M. DUTROCHET.\*

On the subject of my observations on the diurnal variations of the magnetic needle, the observation which I communicated to the Academy at its last session, and which is published in the *Compte Rendu* of that session, I have received from our brother, M. Biot, the following letter, part of which I communicate to the Academy:—

“If you will be at the pains of casting your eye over page 101 of the second volume of the second edition of my *Precis de Physique*, which appeared in 1821, you will there find the exposition of *a process by the aid of which we may increase almost indefinitely the diurnal variations of the magnetic needle, by causing to react upon it another needle equally moveable or fixed.* The same indication is reproduced at page 121 of volume 2nd of the third edition of the same work, which appeared in 1824. In the interval of these two epochs I learned that M. Barlow had read before the Royal Society of London a memoir, containing an account of experiments of this kind, made probably without knowing that the idea had been made known previously, since he did not allude to it. It is for this reason that I thought it my duty to publish, in the *Annales de Chimie et de Physique*, tome xxiv. page 140, a memoir sufficiently extended, wherein I gave the general theory of this kind of reaction, applied to any number of bodies whatever, fixed or moveable, animated by a permanent magnetism.

“If I take the liberty of recalling your attention to these different publications, which appear not to have been known, it is not with a view of making them the object of a personal claim. It is, on the contrary, to thank you for having recalled the attention of philosophers to this mode of experimenting, which, if it was again taken up, under circumstances especially favourable, as indicated by the theory, we should bring to light, I am persuaded of it, many particularities which have escaped us, not only in the phenomena of diurnal variations, but in the reciprocal action of magnetic bodies in general.”

M. Dutrochet deposited a sealed packet, having for its title, *Observations on Electricity.*

M. BECQUEREL presented the following observations on the subject of the communication of M. Dutrochet:—The system of compensated needles, which are made use of for galvanometers, presents the greatest difficulties in its application to the study of the diurnal and other variations of the magnetic needle, not only by reason of the causes signalized by M. Arago at the last session, but also in consequence of the difference of radiation which each needle of the system undergoes from the influence of surrounding objects. It is known that the variations of the temperature modify the mag-

\* Comptes Rendus.

netism of a bar ; if the bar is suspended, the feeble variations which it undergoes do not make it lose its equilibrated position ; and from thence it may serve to observe the diurnal variations. But it is no longer the same, if, instead, we take a system of compensating needles, having only one directing force, extremely feeble, due to the difference of action, and of which one is enclosed in a box, and the other is placed at the exterior. By means of this disposition, the interior needle participates but little in the variations of the temperature to which the other needle is submitted from the action of surrounding objects. From that time, the first sensibly preserves its magnetism, whilst that is not the case with the other. The system is no longer compensated, and tends to lose its equilibrated position : this is a fact, which every one who makes use of a galvanometer has often had occasion to notice. It follows from thence, that the system in question, adapted to the galvanometer, cannot be made use of for observations of the diurnal variations.

*On Secondary Currents.* By M. MATTEUCCI.\*

Since you have had the goodness to communicate to the Academy the extract from my memoir on the *Secondary Current* of the coil, I hope you will further oblige me by also making known the observations I now address to you, and which have been suggested to me after reading the beautiful work of M. Reiss. It is a kind of journal that I send you ; time will not permit me to place in proper order the exposition of my researches. All my efforts have been directed to establish the direction of the secondary current of the coil. Since my first researches, I have always taken for my guide the results of your work, and I have always endeavoured to put myself under shelter of the changes of direction and intensity which occur in taking magnetism as the means *which gives* the direction and the intensity of the current. I am surprised to see M. Reiss, who calls in question this process, rely upon it entirely for pointing out which is the direction of the current. I have made a great number of experiments to establish this action of magnetism, by employing very different charges. The results are constant for feeble charges. We have only to take a coil, containing 400 square centimetres surface of armature, and to charge it from the most feeble degree of tension up to the greatest ; the direct magnetism increases with the tension, and the secondary current is *inverse* in all cases ; that is to say, transmitted in a contrary direction to the current which travels from the positive to the negative surface of the coil. This will occur, at whatever distance we hold the two spiral planes. Cases are cited which cause the direction of

\* Comptes Rendus.

the magnetic fluid to change, without changing the current of the coil. I have found others still more singular. With the small coil above named, the secondary current becomes *direct*, if we cause it to pass through a liquid of small conducting properties, or even if the two extremities of the secondary spiral are held at the distance of two or three millimetres, following the tension. We get in this case a very brilliant spark; but the circumstance that has not yet been remarked, is that in both these cases the magnetism produced by the primitive current is changed: this effect is also produced by interposing the plate between the two spirals. I have made the primitive spiral act on a large disc of pewter, which had at least twenty times more surface than the spiral, and with the method described in my memoir, I have studied the secondary current which is then developed. It is curious to see so great a quantity of electricity put in movement by a very feeble discharge, and on so great a surface; the intensity augments from the centre to the extremities. This movement of electricity takes place, even after having cut a part from this great circle equal in surface to that of the spiral. I have studied the voltaic inductions; they are constant in their direction, which is always the same as that observed by Faraday, at some distances at which we hold the spirals, and whatever be the body which is interposed. A metallic plate diminishes the effect of the induction: for an effect given, it is necessary in the plate to have a thickness as much less as its conductivity is great. By introducing the metallic plate rapidly, we obtain effects which necessarily depend on the action which this plate exercises to reduce the force of this induced current. I am supposing that the plate is such that it reduces almost entirely the induced current. We observe that in introducing this plate, when the circuit is already closed, we have the current in the same direction which we should have observed when opening the circuit, and almost of the same intensity. The contrary would have taken place by raising the plate, always leaving the circuit closed; it is as if the passage of the current had commenced; it is consequently a contrary current which has been produced by induction. When we have observed the first deviation, in having the two spirals immediately one in the face of the other, if, without opening the circuit, we interpose a metallic plate, and afterwards open the circuit, we have a current, the direction of which is always that of the known law of inductions, but the intensity of which is that which we should have obtained if the metallic plate had been put in before closing the circuit. The same occurs if we shut the circuit with the plate interposed, as when we raise the plate; and after we open the circuit, the current of induction which we obtain is that which corresponds to the current of induction obtained without the plate. In fine, yesterday, 17th January, with 4 coils which had 0<sup>m.c.</sup>, 386 of armed surface, I obtained deviations from the secondary current very sensible to the galvanometer. I have employed a galvanometer which has served me in my researches

on the cramp fish ; the wire is covered with varnish, and makes 500 turns. I have also obtained signs on another galvanometer, which is that of Gourjon, and which serves for the experiments of Melloni. The deviations of the secondary current, with these coils charged to  $10^{\circ}$  of the electrometer of Henley, are from  $10^{\circ}$  to  $20^{\circ}$  of the galvanometer. I have in vain tried to obtain deviations by slowly discharging the coil. *The direction of the secondary current given by the galvanometer is always direct, that is to say, direct as that of the coil.* A plate of pewter or of any other metal interposed, destroys the effect of induction ; but the direction of the current is constant ; if, in making the experiment, we hold the ends of the secondary spiral separated by a very small interval, we have a spark ; but what is here remarkable is, the constant *inversion* which we observe in this case of the secondary current. Such is the indication of the galvanometer, and that in every case, whatever be the distance of the two spirals and the interposed plate. The deviation is, nevertheless, considerably enfeebled. I have proved in another manner this direction of the secondary current, when we force it to give a spark.

The spark which I obtain with my two spirals is sufficiently strong to make a hole or pit very strongly marked on paper, and the position of the hole is constant, and is, therefore, a means by which to determine the direction of the current. The hole is always near the negative point, and in all cases, with the secondary current, I have obtained this indication, agreeing with the action of the galvanometer. I avow, and we ought to see it already, if we recal the results obtained with magnetism, that the indication of magnetism does not always agree with the two other processes. The results which I have described to you, however disorderly in their arrangement, are constant ; all have been obtained with M. the Professor Pacinotti, my colleague, to whom I owe many thanks for the active concurrence that he took in my researches.

I have taken great care to conclude that the indication of magnetism is not true ; if the theory of Ampère is a truth, as I believe it to be, the other cannot be so. It is that we are too ignorant still of the nature of a current of an electrical discharge. I believe magnetism to be a means of judging of the direction and of the intensity of the current, more sensitive than any other, and which indicates to us the current for each molecule of the circuit, for each instant, though infinitely small.

Who knows whether the current is not propagated by undulations, or whether by a species of interferences in the circuit ? Who knows if the movement in a circuit is the same at all points where it is broken, where the spark is kindled, and where, if it is not there ? It is for experiment to shew.

## ELECTRICAL CURIOSITIES.

*A Patent granted to H. ELKINGTON, of Birmingham, Gent., for Improvements in Gilding and Silvering certain Metals, &c. Dated Dec., 1837.*

Dissolve an ounce of fine gold in a mixture of “three ounces of nitric acid and three ounces of muriatic acid”—when diluted with *three\** ounces of distilled water. The solution is to be diluted with two quarts of water. The gold is now to be precipitated with a solution of silver in nitric acid and water, and afterwards well washed with water. The precipitate is then to be dissolved in an aqueous solution of common salt, and the resultant solution filtered, (the patentee usually adds equal parts of common salt and borax,) and boiled for a quarter of an hour. When a pale coloured gold covering is wanted, a little chloride of silver may be added. Salts of sodium or potassium may be used, or even sulphates and nitrates, with the borax; but the patentee does not use them.

Another method is to dissolve the gold in the usual *aqua regia*, diluted with water, added to common salt dissolved in water; to which is added five parts of borax and eight parts of salt, dissolved in water: taking care that the solution of gold is first boiled with the borax before the salt is added: afterwards boil the whole for half an hour.

The articles to be gilt are to be well cleaned, and, if small, several may be strung together and placed in the solution, where they will acquire a coating in less than a minute. Such is the method of gilding brass or copper. When silver is the metal to be gilt, the same solution of gold and other salts may be used in the same manner as above; but when the articles have acquired a pale gold colour, they are to be transferred to a similar solution, to which has been added ten times the weight of its contained gold, of alum, and of potash. By this mode a much thicker coating of gold is precipitated.

For gilding iron or steel, the patentee dissolves borax in the above solution of gold, in the proportion of four parts borax to one part of dissolved gold; and when the liquid becomes greenish, he adds four parts of nitre or of common salt. For gilding zinc, the solution of gold and borax only, with some additional water. When the solution of gold is used with ammonia, 20 parts of sal-ammoniac and a  $\frac{1}{20}$ th part of bi-chloride of mercury, for every part of dissolved gold, is to be added, and the whole boiled. Should the solution contain free acid, add carbonate of ammonia. Should the articles, after being placed in this solution, appear of a dull red, a little

\* Our electrical and chemical readers will no doubt pay particular attention to all these *three ounces*, for this point is one of the peculiarities of the patent.—  
EDIT.

bi-chloride of mercury should be added to the solution, and when too pale, too much of the mercury is present.

The patentee, like many others, does not think the proportions that he gives to the public are the necessary ones; for "they may be varied without departing from the nature of his invention."!!!

To silver metallic articles, the silver is to be dissolved in nitric acid, to which solution is to be added as many gallons of water as ounces of silver dissolved. The muriate of silver is formed by precipitation with common salt; and after being dissolved in muriatic acid, boiled for a quarter of an hour, and when cooled, it is fit for use. The articles, when cleaned, are to be suspended in this liquid, and chloride of silver occasionally added, to keep up the saturation. For finer articles, the chloride of silver is to be dissolved in solution of common salt and sal-ammoniac. The patentee goes on again to give proportions of ingredients, such as 1 part chloride of silver, 16 of sal-ammoniac, and  $\frac{1}{16}$  of bi-chloride of mercury; *more or less*, of course, as usual; and as much water as will dissolve the salts; the whole boiled for half an hour, perhaps, *more or less*, and kept boiling when used. Should the coating of silver be found to *peel off*, more bi-chloride of mercury is to be added. The mercury is expelled by heat from the silvered articles, (just in the same manner as has been the practice for centuries,) and they should be quenched in dilute muriatic acid.

Now comes another curious piece of this patentism,—“The patentee states, that under this *head of his invention* he does *not claim in themselves* any of the substances used, metals having before been silvered by the use of salts and chloride of silver; but this has been in a state of *paste*, and effected by *rubbing*; but he claims the application of the *solution* of chloride of silver instead of the *paste*, and *dipping* instead of *rubbing*, for the purpose of silvering.”!!!

*A Patent granted to G. R. ELKINGTON and O. W. BARRATT, of Birmingham, for Improvements in Coating and Colouring certain Metals. Dated July, 1838.*

They are for coating metallic surfaces with zinc, or an amalgam of zinc, to prevent oxydation; and for a method of colouring surfaces of iron or steel so as to make them look like brass.

To protect brass or copper, they are to be boiled in a solution of zinc, in dilute muriatic acid holding metallic zinc, (which process is neither more nor less than making the articles one side of a voltaic battery, and the metallic zinc the other, which has been known and practised many a year ago.)

When iron or steel is to be covered with zinc, they are first to be coated with copper, by immersion in a solution of sulphate of copper, (also a voltaic action.) Then proceed with the muriate of zinc and the zinc plate, as in the previous case. The patentees next find out



that the iron receives a coating of zinc by placing it in contact with a zinc plate, in a muriate solution of that metal. (Cunning men!)

They next turn their attention to an amalgam of zinc; and they think, or pretend to think, that "the zinc should be ground, or otherwise reduced into small particles," which is another great curiosity. The amalgam is placed in a dilute muriatic acid, and the articles to be placed in the liquid, and stirred in contact with the amalgam. Heat is recommended to hasten the process. These men have also discovered that the amalgam may be used on the articles without the muriatic acid and water, if heat be applied. But still they think it necessary that the articles should be previously dipped in a weak acid solution; and still more, they find it necessary that in order to effect a perfect union with the amalgam, these articles must still be held in a solution of muriate of ammonia; so that this *dry* process is still as *wet* a process as the first, and still the same acid used. The patentees then make a guess that "instead of metallic zinc and mercury, oxides of these metals may be used with advantage;" but they make no mention in what way, nor do they appear to know. They next try to hook mixtures of tin and zinc into their wonderful specification, knowing, at the same time, that they have been long used for covering metallic surfaces; and they wish other persons to believe that "part of their *improvements* consists in the combination of this known process with that part of the *invention* above described." But as they do not describe the mode of *combination*, of course they are well aware that they had no need to mention it.

The next part of the business is to colour ferruginous articles so as to look like brass, which is done by first giving them a coating of copper, as before, then a coat of zinc upon the copper, and then fusing these coatings in an oven so as to form brass. Then come the *exceptions* and *confessions*. "They are aware that surfaces have been coated with films of zinc *before*," and they pretend that "such *coating* has merely been used in order to *colour articles of copper by a subsequent application of heat*, which would defeat the object of their present *invention*—(patent, not invention, was certainly what was meant.) The invention of *colouring* metallic articles, these patentees have acknowledged to be *prior* to their patent. "What, therefore, they claim is, firstly, (to be sure) *the mode* above described of coating brass and copper (of all things in the world) with zinc." "Secondly, the *combined* use of zinc and mercury for coating metals to prevent oxydation; thirdly, the *modes*, no less, of coating iron and steel with zinc, to prevent oxydation; fourthly, coating iron and steel, by first covering the same with copper and then with zinc;" which is very easily done in the same solution, and published *ten years* ago at least. Then comes "fifthly, *colouring* iron and steel by coating it with copper, then with zinc, and subsequently applying heat," till brass is formed.

We think it a great pity that speculists, such as these patentees really are, could not see the advantage of consulting scientific men on the subject of their intended patents, before they incurred any expense and risk in their speculations. Had Messrs. Elkington and Barratt been aware of the information that they might have availed themselves of, either by reading the proper books, or by application to persons well skilled in the galvanic process, they might have paused before procuring a patent that may be evaded in many ways.

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*Another ELECTRICAL PATENT is granted to JOSEPH SHORE, of Birmingham, also a Galvanist, of course, for Improvements in Covering and Preserving certain Metals, and Alloys of Metals.*

The articles which Mr. Shore thinks of covering and protecting by galvanism, are of iron, lead, and copper; and the covering is copper or nickel. There can be no doubt of the excellency of this curious patent. Almost every body would suppose that if a *rod of copper* should receive a *coating of copper*, that it would become a thicker rod of copper than before, and, of course, would be very likely to last longer. The nickel part of the business is not amiss. "The patentee does not confine himself to any particular arrangement of apparatus," but, like some other patentees, spreads a net with a view to catch, from the labour of others, all that suits his own purpose. Ignorance and selfishness are the predominating features of many recent patents; and such specifications as these would never have had an existence had the proper advice been given.

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*A Patent is granted to Mr. SPENCER and Mr. WILSON, both of Liverpool, for certain Improvements in the Process of Engraving on Metals, by means of Voltaic Electricity.—Dated October, 1840.*

The metallic surfaces to be operated on are first coated with the common etching ground, and the design drawn, and the ground removed in the usual way, as practised by engravers. This done, the plate is "immersed in a solution of sulphate of copper, and connected with the *negative* pole of a voltaic battery, another plate of copper being placed opposite to it, and in connection with the *positive* pole of the battery;" and the specification goes on to say, that "the effect will be the biting away or corroding of those parts of the metal which have been left unprotected by the removal of the wax with the etching point," &c. Moreover, when cylindrical formed articles are to be *engraved* by this process, "the plate connected to the *positive* pole of the battery should also be of that figure, and placed so as to surround the metal to be engraved, equidistant at all parts of its circumference." When ferruginous surfaces are to be engraved, they are immersed in a solution of common salt, and ano-

ther plate of iron or steel is to be connected with the *positive* pole of the battery. "When the metal to be operated on is silver, a solution of sulphate of soda or sulphate of silver is to be preferred, and a *silver surface* connected to the *positive* pole." Engraving on gold requires the liquid to be "hydrochloric acid, gold forming the connection with the *positive* pole" of the battery.

Mr. Spencer is already aware of our opinion of this patent, and we will only add that we are sorry that these two scientific gentlemen should have gone to the expense of it. The simple error of stating that the metal to be operated on should be connected to the *negative* pole of the battery, is, of itself, fatal to the whole project.\* The fact of metals being dissolved at the *positive* pole, and again revived at the *negative* pole of a voltaic battery, is very old in voltaic processes. The first application of the principle, that we are aware of, was shown by Mr. Mason, at the London Electrical Society, in the process of electrotyping. We have, since hearing of Messrs. Spencer and Wilson's patent, succeeded in making a few etchings by the same process, but no one need expect to succeed by proceeding according to the above specification.

We have a few more electrical curiosities for our next number, and, in future, we shall make a point of placing before our readers all such curiosities as may happen to make their appearance, in hopes that they will profit by our remarks.

## PROCEEDINGS OF THE ROYAL SOCIETY.

(Continued from page 322.)

### 5. *An Account of some Recent Improvements in Photography.* By H. F. TALBOT, Esq. F.R.S.

The author had originally intended, in giving an account of his recent experiments in photography, to have entered into numerous details with respect to the phenomena observed; but finding that to follow out this plan would occupy a considerable time, he has thought that it would be best to put the Society, in the first place, in possession of the principal facts, and by so doing perhaps invite new observers into the field during the present favourable season for making experiments. He has, therefore, confined himself at present to a description of the improved photographic method, to which he has given the name of *Calotype*, and reserves for another occasion all remarks on the theory of the process.

The following is the method of obtaining the Calotype pictures.

\* This error is not peculiar to the patent in question.

*Preparation of the Paper.*—Take a sheet of the best writing paper, having a smooth surface, and a close and even texture.

The water-mark, if any, should be cut off, lest it should injure the appearance of the picture. Dissolve 100 grains of crystallized nitrate of silver in six ounces of distilled water. Wash the paper with this solution, with a soft brush on one side, and put a mark on that side whereby to know it again. Dry the paper cautiously at a distant fire, or else let it dry spontaneously in a dark room. When dry, or nearly so, dip it into a solution of iodide of potassium, containing 500 grains of that salt dissolved in one pint of water, and let it stay two or three minutes in this solution. Then dip it into a vessel of water, dry it lightly with blotting-paper, and finish drying it at a fire, which will not injure it even if held pretty near: or else it may be left to dry spontaneously.

All this is best done in the evening by candlelight. The paper so far prepared the author calls *iodized paper*, because it has a uniform pale yellow coating of iodide of silver. It is scarcely sensitive to light, but, nevertheless, it ought to be kept in a portfolio or a drawer until wanted for use. It may be kept for any length of time without spoiling or undergoing any change, if protected from the light. This is the first part of the preparation of Calotype paper, and may be performed at any time. The remaining part is best deferred until shortly before the paper is wanted for use. When that time is arrived, take a sheet of the *iodized paper* and wash it with a liquid prepared in the following manner:—

Dissolve 100 grains of crystallized nitrate of silver in two ounces of distilled water: add to this solution one-sixth of its volume of strong acetic acid. Let this mixture be called A.

Make a saturated solution of crystallized gallic acid in cold distilled water. The quantity dissolved is very small. Call this solution B.

When a sheet of paper is wanted for use, mix together the liquids A and B in equal volumes, but only mix a small quantity of them at a time, because the mixture does not keep long without spoiling. I shall call this mixture the *gallo-nitrate of silver*.

Then take a sheet of *iodized paper* and wash it over with this *gallo-nitrate of silver*, with a soft brush, taking care to wash it on the side which has been previously marked. This operation should be performed by candlelight. Let the paper rest half a minute, and then dip it into water. Then dry it lightly with blotting-paper, and finally dry it cautiously at a fire, holding it at a considerable distance therefrom. When dry, the paper is fit for use. The author has named the paper thus prepared *Calotype paper*, on account of its great utility in obtaining the pictures of objects with the camera obscura. If this paper be kept in a press it will often retain its qualities in perfection for three months or more, being ready for use at any moment; but this is not uniformly the case, and the author therefore recommends that it should be used in a few hours after it

has been prepared. If it is used immediately, the last drying may be dispensed with, and the paper may be used moist. Instead of employing a solution of crystallized gallic acid for the liquid B, the *tincture of galls*, diluted with water, may be used, but he does not think the results are altogether so satisfactory.

*Use of the Paper.*—The *Calotype paper* is sensitive to light in an extraordinary degree, which transcends a hundred times or more that of any kind of photographic paper hitherto described. This may be made manifest by the following experiment:—Take a piece of this paper, and having covered half of it, expose the other half to daylight for the space of *one second* in dark cloudy weather in winter. This brief moment suffices to produce a strong impression upon the paper. But the impression is latent and invisible, and its existence would not be suspected by any one who was not forewarned of it by previous experiments.

The method of causing the impression to become visible is extremely simple. It consists in washing the paper once more with the *gallo-nitrate of silver*, prepared in the way before described, and then warming it gently before the fire. In a few seconds the part of the paper upon which the light has acted begins to darken, and finally grows entirely black, while the other part of the paper retains its whiteness. Even a weaker impression than this may be *brought out* by repeating the wash of gallo-nitrate of silver, and again warming the paper. On the other hand, a stronger impression does not require the warming of the paper, for a wash of the gallo-nitrate suffices to make it visible, without heat, in the course of a minute or two.

A very remarkable proof of the sensitiveness of the *Calotype paper* is afforded by the fact stated by the author, that it will take an impression from simple moonlight, not concentrated by a lens. If a leaf is laid upon a sheet of the paper, an image of it may be obtained in this way in from a quarter to half an hour.

This paper being possessed of so high a degree of sensitiveness, is therefore well suited to receive images in the camera obscura. If the aperture of the object-lens is one inch, and the focal length fifteen inches, the author finds that *one minute* is amply sufficient in summer to impress a strong image upon the paper of any building upon which the sun is shining. When the aperture amounts to one-third of the focal length, and the object is very white, as a plaster bust, &c., it appears to him that *one second* is sufficient to obtain a pretty good image of it.

The images thus received upon the *Calotype paper* are for the most part invisible impressions. They may be made visible by the process already related, namely, by washing them with the gallo-nitrate of silver, and then warming the paper. When the paper is quite blank, as is generally the case, it is a highly curious and beautiful phenomenon to see the spontaneous commencement of the picture, first tracing out the stronger outlines, and then gradually filling

up all the numerous and complicated details. The artist should watch the picture as it develops itself, and when in his judgment it has attained the greatest degree of strength and clearness, he should stop further progress by washing it with the fixing liquid.

*The fixing process.*—To fix the picture, it should be first washed with water, then lightly dried with blotting paper, and then washed with a solution of *bromide of potassium*, containing 100 grains of that salt dissolved in eight or ten ounces of water. After a minute or two it should be again dipped in water, and then finally dried. The picture is in this manner very strongly fixed, and with this great advantage, that it remains transparent, and that, therefore, there is no difficulty in obtaining a copy from it. The Calotype picture is a *negative* one, in which the lights of nature are represented by shades; but the copies are *positive*, having the lights conformable to nature. They also represent the objects in their natural position with respect to right and left. The copies may be made upon Calotype paper in a very short time, the invisible impressions being *brought out* in the way already described. But the author prefers to make the copies upon photographic paper prepared in the way which he originally described in a memoir read to the Royal Society in February, 1839, and which is made by washing the best writing paper, *first* with a weak solution of common salt, and *next* with a solution of nitrate of silver. Although it takes a much longer time to obtain a copy upon this paper, yet, when obtained, the tints appear more harmonious and pleasing to the eye; it requires in general from three minutes to thirty minutes of sunshine, according to circumstances, to obtain a good copy on this sort of photographic paper. The copy should be washed and dried, and the fixing process (which may be deferred to a subsequent day) is the same as that already mentioned. The copies are made by placing the picture upon the photographic paper, with a board below and a sheet of glass above, and pressing the papers into close contact by means of screws or otherwise.

After a Calotype picture has furnished several copies, it sometimes grows faint, and no more good copies can then be made from it. But these pictures possess the beautiful and extraordinary property of being susceptible of revival. In order to revive them and restore their original appearance, it is only necessary to wash them again by candlelight with gallo-nitrate of silver, and warm them: this causes all the shades of the picture to darken greatly, while the white parts remain unaffected. The shaded parts of the paper thus acquire an opacity which gives a renewed spirit and life to the copies, of which a second series may now be taken, extending often to a very considerable number. In reviving the picture, it sometimes happens that various details make their appearance which had not before been seen, having been latent all the time, yet nevertheless not destroyed by their long exposure to sunshine.

The author terminates these observations by stating a few experi-

ments calculated to render the mode of action of the sensitive paper more familiar.

1. Wash a piece of the *iodized paper* with the gallo-nitrate; expose it to daylight for a second or two, and then withdraw it. The paper will soon begin to darken spontaneously, and will grow quite black.

2. The same as before, but let the paper be warmed. The blackening will be more rapid in consequence of the warmth.

3. Put a large drop of the gallo-nitrate on one part of the paper, and moisten another part of it more sparingly, then leave it exposed to a very faint daylight; it will be found that the lesser quantity produces the greater effect in darkening the paper; and, in general, it will be seen that the most rapid darkening takes place at the moment when the paper becomes nearly dry; also, if only a portion of the paper is moistened, it will be observed that the edges or boundaries of the moistened part are more acted on by light than any other part of the surface.

4. If the paper, after being moistened with the gallo-nitrate, is washed with water and dried, a slight exposure to daylight no longer suffices to produce so much discoloration; indeed it often produces none at all. But by subsequently washing it again with the gallo-nitrate and warming it, the same degree of discoloration is developed as in the other case (experiments 1 and 2.) The dry paper appears, therefore, to be equal, or superior, in sensitiveness to the moist; only with this difference, that it receives a *virtual* instead of an *actual* impression from the light, which it requires a subsequent process to develop.

5. "New mode of preparation of the Daguerriotype plates, by which portraits can be taken in the short space of time of from five to fifteen seconds, according to the power of light, discovered by A. Claudet in the beginning of May, 1841." Communicated by the Marquis of Northampton, Pres. R. S.

"My improvement," says the author, "consists in using, for the preparation of the plates, a combination of chlorine with iodine, in the state of chloride of iodine. I follow the preparation recommended by Daguerre. After having put the plate in the iodine box for a short time, and before it has acquired any appearance of yellow colour, I take it out, and pass it for about two seconds over the opening of a bottle containing chloride of iodine; and immediately I put it again in the iodine box, where it acquires very soon the yellow colour, which shews that the plate is ready to be placed into the camera obscura. I have substituted to the chloride of iodine, chloride of bromine, and have found nearly the same result; but I prefer chloride of iodine, as producing a better effect; and besides, on account of the noxious smell of bromine.

"The result of my preparation is such, that I have operated in ten seconds with the same apparatus, which, without any chlorine, required four or five minutes; when using only the original pre-

paration of Daguerre, I have obtained an image of clouds in *four seconds*."

## PROCEEDINGS OF THE BRITISH ASSOCIATION AT PLYMOUTH, 1841.

(SELECTIONS.)

*On the Influence of the Ferro-Cyanate of Potash on the Iodide of Silver, producing a highly sensitive Photographic Preparation.*  
By Mr. R. HUNT.

The author being engaged in experiments on that variety of photographic drawing which is formed by the action of the hydriodic salts on the darkened chloride of silver, with a view to the removal of the iodide, formed by the process, from the paper, was led to observe some peculiar changes produced by the combined influences of light and the ferro-cyanate of potash. He found that the ordinary photographic paper, if allowed to darken in sunshine and then slightly acted on by any hydriodic salt, and washed, when dry, with a solution of the ferro-cyanate of potash, became extremely sensitive to light, changing from a light brown to a full black by a moment's exposure to sunshine. Following out this result, it was discovered that perfectly pure iodide of silver was acted on with even greater rapidity, and thus it became easy to form an exquisitely sensitive photographic paper. The method recommended is the following :—highly glazed letter paper is washed over with a solution of one drachm of nitrate of silver to an ounce of distilled water ; it is quickly dried, and a second time washed with the same solution. It is then, when dry, placed for a minute in a solution of one drachm of the hydriodate of potash in six ounces of water ; and, being placed on a smooth board, gently washed by allowing pure water to flow over it, and dried in the dark at common temperatures. Papers thus prepared may be kept for any length of time, and are at any moment rendered far more sensitive than any known photographic preparation, excepting the Calotype, which it quite equals, by simply washing it over with a solution formed of one drachm of the ferro-cyanate of potash to an ounce of water. These papers may be washed with the ferro-cyanate, and dried in the dark ; in this dry state they are absolutely insensible, but they may at any moment be rendered sensitive by merely washing them with a little cold water. The paper is rendered quite insensible by being washed over with the above hydriodic solution, and from the photograph thus fixed many copies may be taken. The author then described the action of the spectrum on this preparation, and pointed out that the greatest effect was produced by the least refrangible rays ; but that all the rays, excepting the *extreme red*, acted with considerable energy. The impressed spectrum was, in all cases, distinctly coloured from end to end ; and it was found that the colours of superposed media left a corresponding tint upon the paper, but, unfortunately, as the paper dried, the colours faded. These results bring nearer the probability of being enabled to produce eventually photographic pictures in their native colours. The spectra formed on these papers were all surrounded by a marked space, which was protected from the influence of the dispersed light, exhibiting another proof of the fact noticed before, by Sir John Herschel and the author, that a class of rays having peculiar negative properties emanate from the edges of the sun. Some spectra and numerous specimens of these drawings were exhibited.



*Some Researches on the Developement of the Electrical Force, and an Inquiry into the Nature and Properties of the New Element or product of Electrical Action, described by Schönbein.* By Mr. F. DE MOLEYNs.

The statements made by Prof. Schönbein, at the Glasgow meeting, respecting the production of a new element, which he called ozone, attracted the attention of Mr. De Moleyns; and the paper now read contained some of the more important results of his experiments. In the report alluded to, Prof. Schönbein stated, that the disengagement of the "odorous substance" depended,—1st, Upon the nature of the positive electrodes; 2nd, Upon the chemical constitution of the electrolytic fluid; and, 3rd, Upon the temperature of that fluid. He added, that his experiments went to show that well-cleaned gold and platina were alone capable of disengaging the odoriferous principle, and that the *more easily* oxidable metals, as well as charcoal, did not possess that property at all.—The results of Mr. De Moleyns' investigation appeared to prove,—1st. That the disengagement of the peculiar odour was not confined to the *less easily* oxidable metals; 2nd, That by certain arrangements, *all metals*, when positive electrodes, may be made to develop the odoriferous principle; 3rd, That certain positive metals, not acting as electrodes, will evolve this principle; 4th, That charcoal forms no exception to this rule; 5th, That all substances, whether crystalline in structure or otherwise, possessing the property of appearing luminous by friction, or of yielding sparks when struck, also possess the property of discharging, under such circumstances, the peculiar odour; 6th, That iron and nickel develop this principle more strongly than any other metal. Mr. De Moleyns, observing the odour to be produced at the points connecting an electromagnetic machine with the battery, constructed an apparatus by which magnets were made to revolve within a glass cylinder, which could be exhausted at pleasure, or filled with various gases; by such means he obtained a vacuum, and operated in *dry* air, collecting the matters evolved over distilled water, and by such modes he proved that ozone could not only be produced in a dry atmosphere, but also in a vacuum—mercurial and common. These and other experiments led Mr. De Moleyns to the conclusion, that the ozone of Schönbein, which he proposed to name Electrogen, must be admitted into the list of supposed elements: that it was *not* an union of an electrolytic compound whose cation was *unknown*; and that probably it existed in combination in various forms of matter, which at present are considered, but which in reality are *not*, elementary.

*On Manures, considered as Stimulants to Vegetation.*

By Prof. DAUBENY.

In this paper the author discussed the question as to the sense in which manures can be considered to act as stimulants to plants. It is evident, that if the term stimulus be understood in an acceptation similar to that in which it is employed with reference to the animal economy, it ought to be confined to bodies which, by their presence, assist in promoting the secretion and assimilation of the nutritious materials present, and ought not to include such as themselves afford materials for secretion. Thus, salt and other condiments do not themselves nourish the animal, but, by their presence, induce its secreting surfaces to assimilate more readily the substances presented to them. Now it becomes a fit subject for inquiry, whether manures operate in the former manner or in the latter; and likewise whether the fact, that certain of them act less beneficially at subsequent periods of their application than they did at first, admits of being explained on the recognized principle "that stimuli lose their full effect upon living matter when frequently

repeated." Dr. Danbeny adduced several facts, which led to the inference that the nitrates of soda and of potass operate favourably upon certain crops by communicating to them nitrogen; and that the reason why these salts sometimes have appeared to leave the land in a worse condition than before their use is not owing to their being stimuli, and therefore amenable to the law above quoted, but is because the free supply of nitrogen afforded by the decomposition of the nitrates, had caused the plant to absorb a larger portion of those other ingredients, such as phosphate of lime, silicate of potass, &c., which are present only in a limited quantity in the soil, thus tending to exhaust it of these materials, and causing thereby an inferior crop to be produced on the following year. Now, though it may be true that the nitrates in this manner indirectly stimulate the vital energies of the plant, yet it was conceived that the term stimulus had better be abandoned with reference to such cases, as its adoption might lead to an erroneous impression in the mind of the farmer with respect to the proper mode of restoring to the land its original fertility. If the theory suggested by the author be the true one, it will follow, that the proper remedy would be, not to discontinue the use of the nitrates, but by the application of bone manure, &c. at intermediate periods, to restore to the land those other ingredients which had been abstracted from it in too large a quantity. To determine what materials are wanting, and in what proportions they ought to be applied (independently of the empirical plan of ascertaining, by repeated trials, the substances which, by their addition, succeed best in remedying the deficiency) two methods present themselves. The first, a difficult one, is to learn, by a minute analysis of the soil, whether the ingredients which the crop requires are actually present, and to add of these a quantity equal to that which the intended crop is calculated to contain. The second—a more practical scheme—is to estimate in the first place how much of these substances exists in the crop taken off the ground, and then to add to it at least an equivalent quantity in the shape of manure. The Professor suggested, that a kind of book-keeping should be undertaken in farming establishments on this principle, a debtor and creditor account being made out of the quantity of nitrogen, of earthy phosphates, of alkali, &c. abstracted in the form of crop, and restored in that of manure each year, and concluded by specifying certain points relative to this subject which require further investigation. 1st. To confirm or disprove his theory, with respect to the operation of the nitrates, by determining whether they actually diminish in quantity, and finally disappear after several successive crops have been grown upon land impregnated with these salts. 2ndly. Whether the same applies to common salt and other mineral manures as to the nitrates, or whether any of them act directly as stimuli. 3rdly. More extended and exact data relative to the amount of alkaline and earthy salts, and of nitrogen present in the various crops cultivated by the farmer, as well as in the manures he employs.

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*On the Direct Formation of Cyanogen, from its Elements.*

Mr. G. Fownes, after referring to the experiments of Desfossez, which went to show that gaseous nitrogen brought into contact with charcoal at a high temperature, and alkali being present, is absorbed in notable quantity, and a corresponding amount of cyanide produced; and also to the process for manufacturing Prussian blue, by Lewis Thompson, in which nitrogen is derived from the atmosphere—proceeded to show that the existence of nitrogen in a solid state, in many varieties of charcoal, and the possible presence of ammonia in the nitrogen gas employed, were sources of error against which it was necessary to guard. The author uniformly found, that whenever wood, charcoal, or coke, is ignited in a close crucible with carbonate of potash, at a moderate red heat, cyanide in abundance is always produced, which is never the case with pure charcoal, provided the tempera-

ture does not exceed redness. After some preliminary trials, a mixture of fifty grains of pure sugar charcoal, and fifty grains of carbonate of potash, obtained by gently igniting pure bi-carbonate, was placed in a porcelain tube fixed across a furnace and maintained at a full red heat, while pure nitrogen gas, very carefully prepared by acting on solution of ammonia by chlorine, was slowly passed over the mixture. At the further extremity of the porcelain tube, a small gas-delivering tube was arranged, dipping into a vessel of water. At the commencement, the quantity of gas emitted by the exit end of the arrangement greatly exceeded that passed into the tube:—it had no odour, did not render lime-water turbid, and burned with a bright blue flame, generating carbonic acid. After some time the carbonic oxide diminished in quantity, until at length nitrogen alone escaped. The tube, when cold, being examined, was found to contain a black porous mass, which hissed and became very hot on the addition of water. A little of the filtered solution gave, by "Scheele's Test," abundance of Prussian blue; another small portion, acidulated with nitric acid, gave a copious white precipitate with nitrate of silver, and the residue distilled with dilute sulphuric acid (the addition of which scarcely occasioned effervescence) afforded about half an ounce of tolerably strong hydrocyanic acid. Arrangements were made for repeating the experiment, employing the nitrogen of the atmosphere in place of that artificially prepared, the result of which was, as before, a black mass, rich in cyanide of potassium. The amount of carbonate of potash converted into cyanide by direct absorption of nitrogen, appears to depend very much on the temperature employed. In two trials at a full red heat, the quantity of carbonate converted amounted to 11.5 and 12.5 per cent. of that employed. When, however, the temperature was raised to whiteness, much above the melting point of copper, the production of cyanide appeared much greater. When carbonate of soda was substituted for the potash salt, cyanide was generated, but it seemed with much greater difficulty. The fact, therefore, appears to Mr. Fownes to be established, that free nitrogen can at a high temperature combine directly with carbon, provided some metal, or similar body, be present whose cyanide is permanent under such circumstances.

Dr. Daubeny made a few remarks on the illustration which the facts mentioned by Mr. Fownes afforded of the process by which nitrogen entered into combination with carbon in manure, and also on the probable formation of ammonia in the earth under high pressure.

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*A practical Method of determining the quantity of real Indigo in the Indigo of Commerce.* By Dr. SAMUEL L. DANA, of Lowell, U.S.

Dr. Dana directs that 10 grains of indigo, reduced to an impalpable powder, should be boiled in a Florence flask a few minutes, in  $2\frac{1}{2}$  oz. of a solution of carbonate of soda, making  $30^{\circ}$  to  $35^{\circ}$  on Twaddell's hydrometer, then add 8 grains of crystals of muriate of tin, and boil for half an hour; a beautiful yellow solution of indigo will be obtained. Withdraw the flask from the lamp, and introduce into the solution 500 water-grain-measures of a solution of 50 grains of bi-chromate of potash in 4000 grains of water. The indigo blue, with a trace of indigo red, will be precipitated, while the other components remain in solution. Filter the precipitate through a double weighed filter, washing the mass with 1 oz. of muriatic acid, diluted with 3 oz. of boiling water; wash with hot water till water only returns; separate, dry and weigh the filters; note the weight of the precipitate; burn one filter against the other; the difference is the silica contained in the indigo, which, deducted from the weight of the precipitate, gives the quantity of pure indigo.—Mr. Walter Crum, who communicates the above, adds, carbonate of soda with prot-oxide of tin does dissolve indigo, and forms a yellow solution, but so slowly, that he doubts if all the ten grains are acted upon. He thinks Dr

Dana must mean soda-ash, which contains a notable quantity of caustic soda; but a much weaker solution of caustic soda would answer the purpose.

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*Abstract of a Letter from Prof. LIEBIG to Dr. Playfair.*

This communication announced the discovery of a white crystalline substance, in large quantity, obtained by M. Schunk from the lichens which are employed in preparing archil (*Lecanora tartarea*, &c.) by extraction with ether. It differs from Erythrine and the compounds described by Dr. Kane, in its insolubility in water. It dissolves readily in alkaline solutions, and is capable of being again precipitated by acids, if the solution be recently made; but if kept standing for some hours, acids produce no precipitates: it has been decomposed and is converted into carbonic acid, and *Orceine*. If the substance be dissolved in baryta water, and the clear solution boiled, a large precipitate of carbonate of barytes occurs, and the filtered solution gives, on evaporation, large quantities of *Orceine*. From this circumstance a number of phenomena in the colour of lichens can be explained, which Dr. Kane has described in his work on that subject. Prof. Liebig also states he has performed many experiments on the legumen of beans, and some other leguminous plants. He has arrived at the conclusion, that this body is identical with the casein in milk of animals. It has precisely the same composition, and contains the same salts—phosphate of potash, potash, magnesia, lime and iron—as the casein of milk. The Professor also mentions, that Dr. Will and Dr. Varrentrapp have devised an excellent method for determining the amount of nitrogen in organic bodies. The substance is mixed with a quantity of caustic potash, and hydrate of soda, and heated in an ordinary combustion tube to redness. All the nitrogen in the substance escapes as pure ammonia, which is condensed in a small and neat apparatus, containing dilute hydrochloric acid. This solution is mixed with chloride of platinum, evaporated to dryness in a water bath, and the excess of chloride of platinum is washed from the ammonia chloride by a mixture of ether and alcohol. From the metallic platinum which remains after the ammonia chloride is heated to redness, the quantity of nitrogen is to be calculated. In conclusion, the Professor states that he has repeated all the experiments of Dr. Brown on the production of silicon from paracyanogen, but is not able to confirm one of his results. His experiments prove, that paracyanogen is decomposed by a strong heat into nitrogen gas, and a residue of charcoal, which is exceedingly difficult of combustion.

From the *ATHENÆUM*.

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## PROCEEDINGS OF THE LONDON ELECTRICAL SOCIETY.

We have just now received the first part, in its new form, of the proceedings of this Society, whose welfare we shall always look to with a truly anxious eye. The original form of the *transactions* of this Society were published, at our own suggestion, when a member of the committee, in an elegant quarto form; but whether the transactions be continued in that shape or not, we have no means of knowing at present. The part of the proceedings now before us is in royal octavo, printed by Stewart and Murray, Old Bailey, and is exceedingly well got up, and is probably better suited either for an abstract

of the proceedings, or for the transactions themselves, than any other form in which the labours of the members could be published.

The first article is a letter from Walter Hawkins, Esq., to the Secretary, the purport of which is, the presenting of a dried specimen of the *Gymnotus electricus* to the society. The fish was caught at Espino, and died the latter end of last year, on its transit from thence to Puerto Cabello. Its length is 4 feet 4 inches and a half, and its circumference, at the gills, 11 inches.

The second article is a letter to the Secretary from G. P. Gassiot, Esq., F.R.S., Mem. Elec. Soc., describing a large electro-gasometer, on a new construction. The instrument which is the theme of this letter was suggested to Mr. Gassiot by Mr. Grove, and is thought to be "applicable to every description of battery, peculiarly so as an instrument of measurement for those enormous forces developed by the nitric acid battery." "The base of this" electro-gasometer "is of wood—10 plates of platinum, each plate being 3 inches square, are soldered each to a thick copper wire, and the wires attached to binding screws, arranged alternately—5 on one side of the instrument and 5 on the other. The plates are enclosed in a square glass vessel, on the top of which a very strong glass tube is fixed, bent so as to permit the gases being collected in the usual manner." After pointing out the method of using one pair, or any other number, of plates, Mr. G. says—"In the use of this instrument there is one circumstance which must be borne in mind,—that its power is not in direct proportion to the number of plates employed. If, for instance, one pair of plates is used, the power may be expressed by 9 square inches, the surface of each plate; but if two pairs are in action, both sides of the intermediate plates are excited, and, therefore, the power would be  $18 + 9 = 27$ . And so of the rest."

We are by no means disposed to find fault with Mr. G. for indulging in his philosophy, but we must be permitted to dissent from his conclusions. We had much rather have heard of some *experimental results* than the table of *suppositions* given below. Mr. Gassiot says—"As it is my intention to go through a course of experiments with 100 series of the nitric acid battery, I shall defer my account of effects obtained with the *voltameter* (electro-gasometer) until those experiments are completed."

We are exceedingly glad to find that Mr. Gassiot is about to adopt so prudent a course, for, notwithstanding the tabulated theoretical results might possibly be relied on by certain individuals, we think it much more satisfactory to "defer the account of the effects obtained" until they are obtained.

The ratio of surface *excited* is shewn as under :

	inch.
1 pair,	9 = 1
2 pairs, 9 + 18 = 27 = 3	
3 " 27 + 18 = 45 = 5	
4 " 45 + 18 = 63 = 7	
5 " 63 + 18 = 81 = 9	

The third article is an account of experiments with a constant voltaic battery, in which metals were ignited in an acid solution, by George Mackrell, Esq., Mem. Elec. Soc.

This paper contains an account of the following series of interesting experiments :—The battery consisted of 120 pairs ; a solution, of 1 part of sulphuric acid and 10 parts of distilled water, was placed in a common drinking glass.—Ex. 1. Iron wires, No. 26, were attached to the polar wires of the battery, and that connected with the positive pole was brought into contact with the solution ; the circuit being completed with the negative wire, the latter burned with a reddish flame. The process was reversed, by first connecting the solution with the negative wire, and then completing the circuit with the positive one. The latter became red hot to the extent of  $1\frac{1}{2}$  inches under water, but did not burn. A platinum plate, 1 inch long and  $\frac{1}{2}$  inch broad, was afterwards connected with one of the polar wires ; and, in the following experiments, was always first immersed in the solution.—Ex. 2. The platinum plate was attached to the negative pole, and a small iron wire at the positive one. The wire was rendered red hot  $1\frac{1}{2}$  inches below water, as before ; when in simple contact with the surface of the liquid, it fused.—Ex. 3. The iron burned at the negative pole.—Ex. 4. A fine copper wire, attached to the positive terminal, became red hot about 1 inch under the surface of the solution ; at the negative pole, it burned with a blueish flame.—Ex. 5. A slip of zinc at the positive pole was also heated below the surface ; at the negative side, it burned.—Ex. 6. With platinum at the positive, and sulphuret of antimony at the negative, the antimony fused and inflamed, depositing, on the glass and the surface of the solution, an orange-coloured powder, resembling Kermes mineral ; when the sulphuret was at the positive, white fumes were given off, but it was not ignited.—Ex. 7. With charcoal and iron wire, the former at the negative pole, it became brilliantly ignited, but it did not occur at the positive pole.—Ex. 8. Two platinum plates, of the size before stated, were next used. The negative being first plunged into the solution, on completing the circuit with the other, it became intensely heated, having a blueish light round its edges. On reversing the process, by first plunging the positive plate in the solution, and then bringing the negative one to its surface, no light appeared, but frequent explosions took place. It was observed, that when the circuit was completed by the positive platinum, no gas was developed at the negative one ; nor was there any gas at the positive plate when the negative one was last introduced to the liquid.—Ex. 9. When the solution was put into a glass tube, bent up at both ends, it was made to boil immediately the platinum terminals closed the circuit through the liquid ; and, invariably, the ebullition commenced at the negative end.—Ex. 10. Shewed  $4^{\circ}$  of temperature at the negative end, of a similar solution, above that arrived at the positive pole.

The fourth article is *An Account of Experiments undertaken to*

*Investigate the Nature of the Change of Colour of Bodies by Heat, and their Conducting Power.* By THOMAS POLLOCK, Esq., Mem. Elec. Soc.\*—The apparatus used in these experiments was a glass tube, bent into the shape of the letter U, into each branch of which was inserted a slip of platinum foil,  $\frac{1}{8}$  of an inch broad and 5 inches long, a portion of which was left hanging over the open ends of the tube, for connection with a galvanometer furnished with an astatic needle. A spirit-lamp was also used for heating one or other branch of the tube. The liquids employed in the tube were nitric acid, water, muriate of cobalt, sulphate of magnesia, acid sulphate of iron, chromate of potash, nitrate of iron, nitrate of copper, nitro-muriate of nickel, sulphate of copper, hydrocyanate of potash, hydriodate of potash, muriatic acid, arsenical solution, muriate of iron, muriate of chrome, chlorate of potash, oxalate of ammonia, and chloride of tin.

An extensive table of experiments attends this paper, from which we have arranged the colours of the solutions, with their respective results. There are about ten yellows of different tints, and the deflection of the needle is east in about the same number of cases as it is west. The greens gave west in five experiments, and east in three. The reds, five east and about three west. The blues, in two experiments, gave west deflections.

It would appear from this classification, that colour had very little to do with the results. But, by making a similar classification of the liquids employed, and the application of the heat, the results are such as might be expected; for, in every experiment, there are either *two liquids*, or one liquid at *very different temperatures*, in the two branches of the tube; which, in several instances, became absolutely changed in its character by the application of the lamp. The table of experiments is too long for insertion in this place; we must therefore refer the reader to the work itself.

The fifth article is on *The Effects of Vegetable Points on Free Electricity, and the position they thus occupy in the Economy of Vegetation.* By THOMAS PINE, Esq.—Mr. Pine's excellent views on this subject will be found in the "Annals of Electricity, &c.," vol. iv. pp. 241, 421.

The sixth article is a *Description of a Constant Acid Battery, constructed by the Electrotyping Process; with general Observations on Electrotyping Manipulation.* By CHAS. V. WALKER, Esq., Hon. Sec.—The principal topics of this paper are, *Plaster of Paris diaphragms*, and a copper cell made by the electro-type process. "Take a large jelly-pot, place in it a few pieces of wax, then stand it by the fire till the wax is melted, and the vessel hot; after this, turn it about so that the wax shall adhere to every part of the interior; pour off the remaining wax, and place the jar aside to cool; brush the wax coating with black lead, until well covered. Frequently the black lead used for domestic purposes is best. Pour into the vessel a solution of sulphate of copper, and place in it a piece of amalgamated

\* Mr. Mackrell was also engaged in concert with Mr. Pollock.

zinc with dilute acid in a porous diaphragm. Connect the zinc and black lead, and in a few hours you will have the jar lined with copper, and will form a very efficient *generating cell*."

The next article is a description of *Some Experiments which show that Radiation—a property of Heat—is not a property of Electricity*. By Martyn Roberts, Esq., Mem. Elec. Soc.—The experiments are simply the following:—Ex. 1. "A. BENNET's electrometer was placed at a short distance from the prime conductor of an electrical machine; when the conductor was charged, the leaves of the electrometer diverged; but they again instantly collapsed when the charge was removed from the conductor." Ex. 2. "In order to increase the emissive power of the conductor, a pointed wire was fastened to it, and the electrometer placed at a short distance from the point; the same phenomena again occurred, and this at all distances beyond the actual *striking* distance." Ex. 3. "A glass plate was interposed between the pointed wire and the electrometer." The results as before. Ex. 4. "The electrometer was placed within striking distance of the prime conductor; it now received a permanent charge, the leaves *remaining* separate when the electricity had been abstracted from the prime conductor." Mr. Roberts says that "these experiments tend to prove that the electric fluid cannot be transferred from one body to another by radiation, as is the case with heat, but that actual *connection* must take place. Heat can be transferred by radiation through great distances, and *without affecting the air it traverses*, but the electric fluid cannot be transferred in a similar manner." The author also thinks that "the establishment of facts such as these will tend to throw much light upon the nature of electricity," &c.

We are very sorry, indeed, that Mr. Roberts should have permitted these experiments to have appeared in print, because, although, as made by that gentleman, the results might be as stated, it is well known that an electroscope can be charged at a great distance from the prime conductor of a machine, by the fluid which is thrown off from the latter apparatus.

The next article is *A Translation of a paper entitled, On the Remarks of M. Becquerel, relative to my Comparative Measure of the Action of two Voltaic Pairs, One Copper and Zinc, the other Platinum and Zinc*. By M. M. H. Jacobi. Our own translation of this paper will appear in the December number.

The next article is a description of Mr. Weekes's very excellent arrangement of atmospheric electrical apparatus at Sandwich, already published in the sixth volume of these Annals.

The next article describes some experiments by H. Collen, Esq., with a view to electrotype the Daguerriotype plates, all of which were failures.

The concluding article of these proceedings, in consequence of its great importance, we give entire, as the first of a series which we expect from its highly scientific author.



*A Register of the Electric State of the Atmosphere, in relation to its Meteorological Phenomena, for the month of May, 1841, from observations made by means of an insular apparatus,\* extending horizontally over the town of Sandwich, situate in the vicinity of the British Channel, on the south-eastern coast of England.*  
By W. H. WEEKES, Esq.

NOTE.—The situation of the barometer employed for the purposes of this register, is such as renders the height of thirty inches on its scale exactly twelve feet two inches and five tenths above average high water mark on the adjacent coast.

The thermometer must be understood invariably to be placed in the shade.

When a *single* entry of the state of these instruments appears, on any day, the reference is always to the hour of two, p.m.

No column has been assigned to the reception of hygrometric indications; although it must be acknowledged, from the very important part which moisture performs in the numerous electrical changes of our atmosphere, that such a series of notices would be desirable. I must, however, submit that unless we were furnished with a conventional standard of this kind, as in the thermometric scale, &c., our hygrometers can only apply to individual use, and are very imperfectly adapted to the purposes of comparative observation. I have, therefore, to the best of my power, endeavoured to convey this species of information in text, under the heads *weather, general remarks, &c.*

MAY, 1841.

Day of Month.	Time in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather, and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
1		54	29.72	moderate breeze, N.E. by E.	Clear blue sky.	positive	Free divergence in the electroscopes, with pulsatory and undulating motion of the gold leaves,—ceasing with the wind and setting sun.
2		50	29.10	gentle gales, S.W.	Constantly clouded by cirro-stratus, &c.	negative	Few symptoms of electric action.
3		48	29.05	none	Continuous gentle rain.		Even the most delicate species of test-instrument remains inactive. Scarletina and influenza are prevalent.
4	2, 30 a.m.	42	28.90	gale, W.N.W.	Hasty drenching shower.	positive	Dense sparks streaming from terminus, with intermissions of a few seconds.

\* Vide Proceed. Elec. Soc., Session 1841-2, p. 41.

Day of Month.	Time, in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
4	10 a.m.	46	29.00	breeze, S.W.	Various modifications of fleecy cirri,—an under stratum of haze.	positive	Slight occasional divergence.
5		49	28.97	moderate S.W.	Dense clouds and some small rain.		No obvious electric action.
6		52	29.20	slight, N.W.	Light fleecy cirri, and some haze.	fluctuating from neg. to pos. suddenly positive	Strong divergence; the gold-leaf pendulums strike frequently, and pulsate at intervals.
7	8 p.m.	56	29.70	breeze, S.W.	The cirrus comoides frequently passes slowly over.	positive	Slight divergence at this time only.
	night			increased N.W.	Uniform dense cloud.	positive	Continuous low current, yielding smart shocks at times.
8		48	29.10	smart breezes, N.W.	Numerous modifications of cirrus cloud.	variable	Frequent heaving and, as it were, troubled motion of the electroscopic pendulums.
9		46	28.30	moderate S.W. by W.	Cumulo stratus in western horizon.		No trace of electric action is manifest.
	noon				A magnificent storm-cloud hangs over the line of wire, discharging tremendous showers of immense hail-stones, drenching rain, and repeated lightning flashes.	regularly alternating from pos. to neg.	Splendid and nearly incessant torrent of sparks for upwards of an hour. The electric current exhibits singular intensity, rapidly decomposing fluids, igniting combustible substances, &c. Lateral discharge partially consumed an iron nail 34 inches in length, with the brilliancy of an ordinary fire-work.
	2 p.m.			breeze, S.W.	Occasional showers of fine rain.	negative	Very slight divergence on commencement of each succeeding shower.
	evening			none	Clear sky.		Apparatus silent.
10				moderate	The ordinary modifications of cloud,	positive	No remarkable phenomena, except slight divergence in the electroscopes at the commencement of a shower.
11		54	29.75	breezes, S.W. & W.	with an occasional shower of fine rain		
12					Clear blue sky.	positive	Uniform free divergence.
13		47	30.00	strong breeze, N.E. by E.			
14		43	30.09	increased breeze, E.N.E.	Clear blue sky.	positive	Frequent sparks and strong shocks from terminus. A tonic effect to the animal system is felt. Asthmatic and rheumatic people complain of aggravated symptoms.
15		47	30.00	slight breeze, N. E.	Transparent blue sky.	positive	Constant slight divergence.

Day of Month.	Time, in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
15	11 a.m.			increased		positive	Perpetual heaving motion of the gold leaves.
	6 p.m.			undiminished		positive	Energy of the current has been uniformly increasing. Sparks and strong shocks are frequent.
	sun set			none			The electric current has suddenly ceased with the wind. Serene moonlight.
16		45	29.30	breeze, S. W.	Clouds with an under stratum of thin haze.		No evident electric action.
	3 p.m.	40	29.10	increasing N.W.	Cumulo floccosus,—the haze has dispersed.	negative	A very moderate current occurs at long intervals.
17		42	28.90	a moderate gale, S. W.	Haze and flying clouds.	negative	Very slight divergence.
	evening			slightly decreased	Some fine rain.		Apparatus silent.
18		42	28.30	strong S. W.	Heavy showers.	variable	An occasional vigorous stream of sparks commencing with a driving shower, and ceasing after a few seconds; the rain continuing notwithstanding.
19		40	28.35	frequent gales, S. W.	Much rain at times.	positive	A weak current almost uniformly until noon, when all electric action apparently ceased.
20		44	28.90	uniformly strong, S. W.	Haze and fragmentary clouds.		No manifestation of free electricity. Neuralgic pains are prevalent.
21		43	29.70	smart breeze from eastward	Light haze, and clouds at a considerable elevation above.	positive	As usual during easterly winds the electroscope diverges to its fullest extent. Slight shocks from terminus occasionally.
22		46	29.75	breeze, N.E.	Loose masses of flocculent clouds at great elevations, with intermediate haze.	positive	A moderate current, nearly uniform, until cessation of wind at evening.
23		49	29.40	strong bleak breeze, N.N.W.	Gloomy—uniform sheet of cirro stratus.		Apparatus silent throughout.
24 } 25 }		56	29.43	moderate and variable from N. to N.W.	Continuous dense haze upwards		During these two days not even the large plate of an excellent condenser, brought in contact with the terminus, could induce divergence of the electroscope.

Day of Month.	Time, in hours and minutes.	Thermometer.	Barometer in inches and hundredths.	Wind, its direction and character.	Weather and modifications of clouds.	Electrical condition of atmosphere.	General Remarks.
26	sun rise	52	29.50	gentle breeze is springing up from N. E.	Haze is fast dispersing.	positive	Constant divergence throughout the day, with passage of minute sparks frequently, until the wind ceased at sunset.
	noon	74		by E. smart breeze from eastward	Clear blue sky.	positive	
27	sun set	73	29.45	breezes S. W.	Clear and very fair	positive	Morning and evening divergence.
	10 p.m.				Beautiful modifications of feather-formed cirri.		Apparatus silent.
	11,30 pm.				Cirro cumulus in the horizon.		The electric flash is too distant to affect the apparatus. The air here is in a neutral condition.
					Distant lightning.		
28	3 a.m.			strong S. W.	Heavy shower.	negative	Aroused by violent commotion of electric chimes; torrent of powerful sparks.
		72	29.62	gentle breezes, S. W.	Clear and very fine		Every form of electroscope obstinately quiescent throughout the day.
29	1 p.m.	67	29.65	none	Thin veil of cirro-stratus.	negative	Apparatus still quiescent.
					Occasional rain drops.		Slight instant divergence when rain drops commence. Sensations of extreme lassitude, and weary pains in muscles of feet and legs.
	7 p.m.				Hasty shower during five minutes.	positive	Current of small sparks.
30	evening	66	29.63	scarcely appreciable N.W.	Gentle rain at times	positive	Low current nearly uniform.
					Clear sky.		Apparatus silent; — Æolian sounds proceed from the line of wire.
31		64	29.64	zephyrs, W.	Fine, with cirrus clouds.	positive	Slight divergence during transit of large masses of cloud.

*Photography. New Discoveries of M. DAGUERRE.\**

M. Arago announces that he is actually authorised to communicate to the Academy the methods which M. Daguerre has discovered, to give to photographic processes an inconceivable rapidity. The Secretary has not personally witnessed, at present, the products of this new art. All that he has to do, then, is to confine himself, on his part, to the most faithful reproduction of his verbal conferences with the ingenious artist. M. Daguerre, adds M. Arago, does not conceive that he has at present pushed these processes to all the perfection that is desirable, particularly if viewed in an artistical point of view ; but he considered it desirable to give way to the impatience of the public, and to furnish, without further postponement, the fundamental principle. The way being open, M. Daguerre advances in the most praiseworthy manner, without any reserve, to detail the success of these experiments to those who will judge these new phenomena worthy of their investigation.

M. Daguerre, having insulated, and afterwards electrized the plate of iodised silver, of which he made use in his former methods, acknowledges that by this means the sensibility of the impressionable coat is prodigiously augmented. It is sufficient then, in fact, to create the images which the mercurial vapour afterwards renders so apparent, to raise the screen, and to let it fall with rapidity, immediately after.

In the application of this process he has only produced disordered images, striated, and without harmony. This want of success may be explained, by remarking that the inferior part of the focal image receives the light during a longer period than the superior part ; that the edge of the screen, during its descent, projects successively rays defracted within and without ; that the movement of this screen, inasmuch as its operation is carried on through two grooves, and that however rapid it is, its motion cannot be uniform, that inappreciable periods of rest, as well as imperceptible changes in the quickness of the motion, ought, inevitably, to be accompanied with corresponding times of rest, and similar changes of speed in the march of the defracted light. One circumstance seems to come to the support of this explication ; when the inferior part of the screen is crooked, the furrows which traverse the photogenic images here and there are crooked themselves.

The production of perfect photogenic images having miscarried, as we have just seen, by the excessive sensibility of the electrized impressionable coat, M. Daguerre ingeniously conceived the idea of trying with matters of a less degree of sensibility, and of not insulating or electrizing the plate in the focus of the darkened chamber, except for a single instant, that is to say, by the aid of a simple spark.

\* *Comptes Rendus.*

The experiment thus executed, succeeded; the matter became very susceptible of impression at the moment the electrical discharge was achieved, and the exceeding short duration of this phenomenon did not hinder the image in the focus of the darkened chamber from being produced, and fixing itself as in the older method.

Any one will comprehend that in this second mode of experimenting, the movements of the screen may be, comparatively, a very little more rapid, without any appreciable inconvenience.

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## ELECTRIC STORMS.

In our last report of electric storms, in the September number of the *Annals*, we have mentioned, at page 249, that "on Tuesday morning, August 24th, the lightning struck the steeples of two churches in Liverpool." We now know that these churches were St. Michael's, and St. Martin's-in-the-Fields. The following interesting particulars we have collected, principally by the kindness of Mr. Dircks and Mr. Smith, of Liverpool:—

Monday, August 23rd, was tolerably fine, but the weather was capricious and changeable, and apprehensions of rain were very generally entertained. About nine o'clock these prognostics were fulfilled. A small quantity of rain fell, while it became evident, from the numerous gleams of evanescent light which, ever and anon played over the atmosphere, that the air was surcharged with electrical matter.

About two o'clock on Tuesday morning there passed over the town a storm of thunder and lightning, of short duration, but of fearful violence, which has proved exceedingly disastrous in its effects. The lightning broke forth twice only, and each time the immediate roll of the thunder, "in explosion vast," raising its tremendous "voice" amid the silence of the morning, indicated the proximity of the danger. The rain which followed was a complete deluge. The water poured from the clouds in streams, and the low parts of the town were completely flooded. The storm came on very suddenly. A long, distant rumbling had, indeed, warned the wakeful that a thunderstorm was traversing some adjacent district. Suddenly, about the hour above mentioned, a vivid flash of lightning suddenly illuminated the whole firmament. A clap of thunder, which made the earth tremble, followed in a few seconds. The first explosion was echoed and re-echoed in a series of stunning reports, which were, in the next instant, changed to a prolonged roll, that gradually died away in distant reverberations. In a few minutes, a second flash, more vivid than the first, lighted up the atmosphere. The thunder almost accompanied it, bursting on the ear with even greater intensity of sound than the former peal, though it was of shorter duration. Thousands upon thousands were startled

from their sleep by the first awful sound, and, in the twilight of scarcely awakening consciousness, were filled with vague apprehensions of evil. Ere they had time to reason upon what had occurred, the second flash fell upon the startled eye, and the terrific peal which followed left them without doubt as to the cause which had disturbed them.

"The storm," says the *Standard*, "as experienced by those on board ship in the river, was at once sublime and terrific. Lieutenant Wilson, commander of H. M. S. Etna, lying in the Sloyne, describes the storm in a most interesting manner. He states, that the first flash of lightning appeared to strike the Mersey opposite the town, and the Etna vibrated from the mast-head to the keel, so that all on board conceived, for a moment, that she was shaken to pieces.\* The parties on deck (including Captain Wilson, who kept his own watch) lost their sight for several seconds, but on recovering found that no damage was done to the ship. The second flash appeared to evolve from the southern extremity of a dark cloud, and flashed from one extremity of the town to the other." One or both of these explosions of electricity unfortunately did serious mischief, having greatly injured the steeples of two churches, one at the southern and the other at the northern extremity of the town, three miles apart.

The steeple of St. Michael's Church, which is one of the handsomest in Liverpool, has sustained injuries so considerable that it will have to be taken down and rebuilt. The spire is now merely, to use the words of Sir Walter Scott, "a thunder splintered pinnacle." The steeple is erected on a pedestal which rises from the roof, and from which spring sixteen Ionic columns which support a balustrading, and another pedestal surmounted by a similar erection in the Corinthian order, from which rises an octagonal spire, finished at the top by a capital. The total height of the steeple from the ground is 201 feet. The spire is now little better than a ruin. The lightning appears to have struck the spire near the top, about one-fourth of the distance between the apex and the base. It seems then to have ploughed the masonry in a zigzag direction towards the bottom, tearing out the stones in its progress, splintering the strong stone ribs by which the corners were finished, till at length it rooted out a mass of masonry at the base and then entered the steeple, which, thus undermined and shattered, looks as if the slightest shake would topple it over on the roof below. The shock must have been most violent; and there is little doubt that, had it not been for the great strength of the work, the spire would have been overthrown. The upper part is of solid masonry, through which an iron rod runs to the apex. The stones below are all strongly fastened and clamped with iron. The electrical matter, after entering at the base of the spire, seems to have descended through the interior, doing great damage in its progress, having considerably injured the tower,

\* We hope that this fact will be particularly noticed, as it shows that a vessel being *shaken* is no evidence of its being struck by lightning.—EDIT.

till it made for itself a place of exit, by forcing out a large stone from the pedestal, beneath the clock, on the east side. The works of the clock are on this side, and the lightning seems to have passed out beneath them. The clock had stopped at twenty minutes past two, and this is supposed to indicate the precise time when the lightning struck the steeple. Some of the stones which were forced from the spire were of large size and entire. Others appear to have been broken off in huge fragments, and the course of the lightning is indicated by a blue discolouration. Several heavy masses of stone fell upon the roof, which was broken, but none of the fragments fell through. Other pieces were hurled into Pitt-street, and into the churchyard, where they have, in one or two instances, smashed through the flags. Some smaller fragments have been forced out with so much violence that they have been projected over the flagged portion of the churchyard to the greensward, which is railed off, and there they lie, completely buried in the turf by the force of the descent. The shock and the noise of the falling stones were, we understand, heard by several of the families residing near the church.

The reporter for the *Standard*, who surveyed the interior of the church, gives the following account :—"On the roof, the colonnade, on the north side, projecting continuously with the roof of the church from the base of the steeple, large fragments of stones are strewed about, and five large holes are made by others through the slates and wood-work, as if produced by cannon balls. A portion of the capital of the Corinthian columns in the upper story of the steeple has imbedded itself in the lath-work under the roof. Several holes are also formed in the roof of the church on the same side by the stones that have fallen, and the rain has, consequently, come through the roof of the aisle in several places. The electric fluid appears to have escaped from the steeple by the stone-work immediately under the east clock and close to the ridge of the church roof. Several stones are there much broken, and outwardly abrupted, as if caused by an explosion from within. In the interior of the steeple pieces of the broken stone from the spire above are thickly strewed in the bell-loft. One of them, apparently of near a hundred weight, has, in its fall, struck the axletree of the bell, and the strong frame in which it hangs. The bell, which is about one yard and a half in diameter at the mouth, was driven from its pendant position on one side of the frame-work, and now rests on that side, on the floor, surrounded by broken stones and rubbish. From this point the steeple was visible interiorly to its top. One of the huge stones in the western arched window of the bell-loft is loosened from its position, and considerably broken. There are, also, slight appearances of the electric fluid having here and there touched the stones in the interior of the tower; but nothing of consequence. The building generally, indeed, with the exception of the spiral part, may be considered uninjured."

While it is a subject for congratulation, that the lightning did not occasion any loss of life, we have heard many expressions of regret



that it did not rather expend its terrific power on some meaner object, and damage some less majestic and less sacred edifice.

Ever since the accident crowds of people have assembled in the vicinity of the church, led by curiosity to gaze upon the destruction which had been so suddenly wrought. Policemen are in attendance to preserve order, as well as to prevent the populace from passing along that part of Pitt-street which joins the churchyard, as it is deemed that there is some danger of the spire falling. For the purpose of keeping this portion of the street clear, chains are extended across at each extremity, and the officers are directed not to permit any person to pass. Workmen are employed in the church, making preparations for the erection of scaffolding. The operation of taking down the spire will be one of some difficulty.

The spire of the church of St. Martin has also suffered great injury; but, as the edifice is not so handsome or costly as that in Pitt-street, and situate in a neighbourhood inhabited by the working classes, the disaster has not excited so much interest. The lightning has struck the spire just at the first string of beading, about ten feet from the apex. A large hole, several feet in circumference, has been made by the electric fluid, and the top of the spire is now supported by no more than two-thirds of the stones forming the circle. The displaced stones fell on the roof, which they broke through, and damaged the organ and the interior of the church.

The following remarks on the effects of the lightning on the spires of St. Martin's and St. Michael's Churches, are from the pen of a gentleman who has surveyed them:—

“EFFECTS OF THE LIGHTNING ON THE DAMAGED SPIRES.

“The upper portions of both spires are of solid masonry, having a metallic rod, either of copper or iron, running through the centre, and screwed to cross-bars at the bottom of the solid portion. The object of this is to secure all the upper stones (and, in the case of St. Michael's, the ball and cross) the entire depth of such solid portion. The remaining portion of the spires are composed of ashlar stones, nine inches in thickness, on the bed, and are cramped to each other with copper cramps and run with lead. Such being the construction of the spires, let us see if we can account for the fact of their being so shattered by the lightning. The upper portions of both, where the metallic rod is continuous, are not damaged; but the mischief commences below, and some of the cramps have been blown entirely out of the solid stones into which they were bedded. If any thing would shew the want of proper conductors, the tower and spire of St. Martin's Church, I think, furnish sufficient evidence of the fact. There cannot be any doubt but that the electric fluid took the point of the spire first, following the metallic rod before spoken of, which carried it safely through that portion into the masonry below, where the damage commences, and is the most extensive: had there been a proper metallic communication to the ground, the probability is, little

or no damage would have been sustained. The spire of St. Martin's, where it springs from the tower, has a balcony round it, which is leaded: from this flat there is a lead spout goes down one of the internal angles of the tower, bending through the lower window on the roof of the southern entrance to the gallery, which is a lead flat also. The lightning, after striking this spire, has shattered it more or less from the upper solid portion down to the lead flat at its springing; there it has blown a portion of the internal flashing out, torn a portion of the lead at the doorway, taken the lead spout, bursting every joint in its course, and breaking a few squares of glass where it passes through the window, but leaving the masonry unblemished through the whole space occupied by the spout. It appears to have been distributed over the outside lead flat, and to have passed off without any further damage to the ground. There is a fragment of brickwork blown out of the wall over the stairs on the opposite side of the tower, and there is a small lead pipe comes in there to one of the gas lamps. This circumstance shows, that the whole substance of the tower was more or less charged with the electric fluid. It is to be hoped, as both spires are about to be taken down, that precautionary measures will be adopted to guard against accidents of so frightful a nature, by introducing conductors upon the most improved principle upon the rebuilding of them.

“R.”

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ST. MICHAEL'S SPIRE.—VIEW FROM THE TOP.

LIVERPOOL STANDARD, 24TH SEP., 1841.

The scaffolding erected preparatory to the pulling down of the spire surmounting the tower of St. Michael's Church, which was shattered by the lightning on the morning of Tuesday the 31st ultimo, having been completed up to the ball and cross, we were last week induced to ascend to the summit, in company with a friend, to obtain a bird's eye view of the town and the surrounding country. The day was fine, and though the prospect was slightly dimmed by the smoke that hung over the dense masses of buildings, and the exhalation from the suburban fields evolved by the ardent beams of the sun, the panoramic prospect, bounded by the distant circle of the horizon, amply repaid the slight toil of attaining an elevation to which we had never before *aspired*.

The ascent was somewhat difficult, and, it may be added, occasionally perilous, for those who, like ourselves, are “all unused to the *climbing* mood;” and a short previous apprenticeship to a slater would be no indiscreet preliminary to the uninitiated who may undertake a similar adventure. We mounted by two flights of pretty long ladders to the roof of the church at its south-east corner, an intermediate platform being formed by the leaded top of the vestry-room. A walk along the leads within the blocking, and thence to

the middle of the roof at the west end of the church, brought us to another long ladder reaching to one of the large lower windows of the tower over the clock; and another, inside, led us into the bell-loft. Successive ladders, with temporary small scaffolds between them, took us to the height of the lower tier of oval loop-holes above the base of the spire.

In our progress we marked the apparent direction and destructive effects of the electric fluid, which appears to have first struck the spire, which is octagonal, near its top, on one of the mouldings at the south-west angle, and probably passed down it in a somewhat spiral direction, shattering much of the stone, breaking large fragments out of it in several places, and leaving irregular rents and several large holes freely admitting the light. The largest of these apertures is close to the spring of the spire, on the north-west side. Several large pieces of stone "shot" from their position near the top, fell upon the framework of the large bell, dislodging it on one side from the socket or step on which it swung, so that its lip on the south side rests on the floor. The lightning, after passing down through the steeple, here and there abrasing the stonework in its impetuous progress, appears to have passed out through the thick wall, between the ridge of the church roof and clock face, on the east side of the tower, bursting away exteriorly considerable portions of the large blocks of stone. It is worthy of remark, as exemplary of the extraordinary freaks of lightning, that one of the large stones about half way up the spire is split in two, in a vertical direction, in the centre of the wall, and one half of it forced out an inch or two at one end, and the other equally projected interiorly, in the form of the letter < reclined, or rather a more acute angle. Neither of the halves is otherwise broken, so that an angular cavity is left between them.

Emerging from the interior by creeping out on our hands and knees through one of the lower loop holes above the base of the spire, we got upon the first or lower floor of the exterior scaffolding, which rises to the top in the conical form of the building, encircling it in eight successive stories or heights, each of a little above two feet wide, and railed and stayed exteriorly. The work of erecting these was, from the shattered and insecure state of that part of the building, a work of considerable difficulty and danger; but it was happily accomplished without accident, under the immediate superintendence of Mr. Shadrach, foreman of Messrs. Foster and Steward.

The carpentry is ingeniously contrived: each scaffold is secure in itself, forming as it were a hoop round the spire, tightly fitted, so that if the wood below any one of them were removed, it would still securely stand, the weight being almost solely on the spire itself. It became necessary, however, for the workmen to secure the stone work where it was dilapidated, by upright planks laid flat upon it here and there. These form, as it were, huge staves, and the scaffolds being the hoops that secure them; the shattered fabric

may be said to be "coopered up," in a manner sufficiently secure for the purpose of taking it gradually down. We observed also that, for additional security, several of the rents and holes in the masonry are temporarily made good with brick and Roman cement.

The view from any of the scaffolds is fine, but that from the top, which we reached by a succession of small ladders and little hatchways at the top of each, is of course, from its elevation, the most extensive. The height of the tower is 157 feet 8 inches, and that of the spire 54 feet 10 inches, exclusive of the cap and cross, which are 9 feet 6 inches, making the entire height of the building to the cross 222 feet from the street. We mounted to the ball, and finally sat and stood on the arms of the cross, which, though it appeared small from below (for it is since removed), was three feet eight inches in height over the ball, and three feet five inches across. The diameter of the ball was one foot nine inches. Both were supported, though in rather a fragile manner, by a vertical copper rod,  $1\frac{1}{2}$  inch in diameter, and wormed so as to screw them down from the interior. This rod penetrates the crown stone, and the solid stone work, which is 15 feet in height.

We may here remark that the spire is to be rebuilt precisely of its original height, and in the same form, but of superior material and workmanship. The masonry, indeed, of the steeple (as we noticed at the time of the accident that befel it), was defective from the first, the stone being poor and soft, and withal insufficiently finished and jointed for a work so much exposed to the weather. The ashlar wall of the spiral part is nine inches thick, and is but roughly dressed inside with the pick. It was built by contract, without being subjected to the wholesome surveillance of a competent surveyor, as our present buildings properly are. Since there appears to be a difficulty in securing the top part of our spires, we do not see why light bronze castings might not be substituted for stone for fifteen or twenty feet of the attenuated part, secured by an upright strong rod, socketted into a beam of timber reaching down to the tower, and stayed interiorly to the walls. The top could not then be unsettled by gales of wind, unless the tower itself gave way, which is improbable; and we might then have the advantage of a weather-cock, that best and most useful finish to a spire, which our modern ecclesiastical architects appear to be afraid to affix.

Considerable vibration of the cross and the slender stonework below was caused by our movement upon it. Indeed, the whole, to a considerable distance down, could be sensibly put in vibratory motion by the hand; and our companion—very wisely, we must say—declined ascending to the "tip-top."

#### FURTHER PARTICULARS.

As soon as I heard that the scaffolding at St. Michael's Church was completed, I proceeded to Liverpool to examine the damaged edifice; and, by the assistance of the workmen, I was fortunate

enough to arrive at the top of the spire. During our ascent my attention was frequently directed to the havock which the lightning had made on various parts of the interior of the masonry, at which, at that time, I merely took a cursory view, with the intention of examining the particulars more closely as I returned.

On reaching the top, I first examined the metallic cross, and the ball, in which the lower end of the shank of it was fixed, and I very shortly discovered, by the discolouration of the metal, which is bronze, that the electric fluid had first struck the lower end of the shank of the cross, and that it had not touched the upper part of it at all. The ball or cap, and cross together, are 9 feet 6 inches high, and I estimate the cap, which is a hollow ball of bronze, to be about two feet and a half diameter, which would leave 7 feet for the height of the cross above the surface of the cap. A more decisive indication of the precise spot where the electric fluid first struck the cross, could not possibly have taken place, than by the well-defined boundary of discolouration of the bronze. And what was more satisfactory still, the *obliquity* of this upper boundary line, or margin of discolouration, shewed, in the most decisive manner, that the cloud from which the lightning proceeded was, at the time of the discharge, on the north-west side of the church. For on that side of the shank of the cross was the highest point of the margin of discolouration, and on the opposite side was the lowest point of that margin; and the figure of this upper margin of discolouration was something of an ellipse placed round the shank of the cross, sloping downwards from the north-west to the south-east side.

The highest part of this boundary or margin is not a foot and a half above the surface of the cap, leaving above five feet of the best conducting metal, above that point, untouched by the lightning. The gilt bronze from that margin down to the horizontal equator of the cap is converted into a leaden blue colour. At the equator of the cap the discolouration becomes scattered, and lost in a multitude of ramifications on the lower half of the ball. The ramifications are precisely similar to those made on the surface of a glass jar, by spontaneous discharges. This is one of the most striking evidences that has come to my knowledge, of an *oblique* discharge of lightning striking a good conductor at so great a distance from the highest point.

From the cap descends a stout cylindrical metallic bar, which I was told is either of copper or bronze, (gun metal,) but which, more probably, is iron. The spire, with the exception of a few feet at the top, is hollow, and the bar proceeds downwards nearly 40 feet, and rests in the intersection of two stout flat iron bars, crossing one another at right angles; hence, from the shank of the cross, where the lightning first struck, to the bottom of the central rod, no damage was done to the spire. But, as the lightning no longer found a continuous conductor, it commenced its destructive effects on the masonry, at the extremities of the cross iron bars on which

the central bar rested. I cannot say that the effects produced were different from what might have been expected, from a heavy flash of lightning striking an edifice whose materials were similar, and similarly distributed, to those which constituted this steeple. The stones are held together by copper clamps, and the spare lead which had been left in the ladles, after soldering the clamps into the stone, seems, whilst yet in fusion, to have been carelessly poured into the crevices between the stones; forming, when cooled, solid bars of lead, of considerable dimensions, both in length and breadth. Several of these masses of lead were completely dislodged by the electric force, and many scores of pounds might have been easily removed by the hand only, from the rents made in the spire. I brought a few pieces with me, which are now lying on one of the tables of the gallery of this Institution.

The effect, generally, below the iron rod, would seem to have been accomplished by a formidable expansive force in the interior of the spire, which has operated in two diametrically opposite sides, to the greatest extent; or, at least, the effects produced are greatest diametrically opposite each other, in all those places where absolute rents are made in the masonry; and the rents themselves are such as to indicate an expansive force from within. In several places, however, large masses of masonry are split off the inside of the spire, but by far the greatest quantity of shattered stone is carried outwards. There are some vertical rents at the octagonal angles, more than 20 feet long; and in all cases where there is a rent on one side, there is another on the opposite side of the steeple.

The axle of the clock bell is broken, and the bell itself leaning on the loft floor, probably in consequence of a large mass of displaced masonry falling upon it: but there can be no doubt whatever of the lightning making a stepping-stone of this bell, to facilitate its transit from the last place of damage above it to the metallic works of the clock below. From the works of the clock the lightning has darted through the wall close to the lower edge of that face of the clock, which is over the body of the church, and has peeled a considerable slice off the outer side of the stone work. This appears to have been the last point of destruction; from which it no doubt darted to the lead work of the roof of the body of the church, and thence to the ground by the conduction of the metallic water pipes.

In conclusion, I may add, that had the architect been disposed to build an edifice for the precise purpose of being destroyed by lightning, he could scarcely have improved upon the plan exhibited by this spire, either as to the choice or the distribution of the materials.

*Remarks.*—Now, as it appears that these elegant tall thin spires cannot be held together against the force of heavy winds, independently of metallic clamping, and as we have now, I hope, sufficient evidence of the danger of this plan, should lightning happen to strike the upper part of the edifice; and also, as great facilities for such events are afforded by the distribution of metal amongst the

masonry by such mode of protection against less formidable elements, it behoves those who, in future, may have the planning and erecting of tall spires, to provide especially against the effects of lightning.

Now, it appears to me, that had the cloud which discharged the lightning against St. Michael's Church, been at a less angle of elevation, the cross would not have been hit at all; but that the lightning would, more probably, have struck some part of the central metallic rod below; under which circumstances the masonry must have been penetrated before the rod was arrived at; for lightning regards not stone walls when they happen to be in its way.

From these considerations, it would appear that a lightning rod passing through the interior of a tall spire, although it were exposed at the top, and also in good connection with the ground at the lower end, would not give complete security to the building, especially against oblique discharges, when the angle of descent was very low. A single exterior rod would be preferable, because if the lightning happened to come from a cloud on the same side of the edifice, the rod would be struck before the lightning arrived at the masonry; but a single exterior rod might possibly be the means of much damage from lightning which approached from the opposite side. Therefore, if we are disposed to give decided protection to such edifices from the destructive forces of lightning, *three rods*, at least, at equal distances from one another, ought to be exteriorly arranged from the top to the bottom of the spire, and continued down to the ground. Such an arrangement of cylindrical copper rods of moderate dimensions would operate in concert, by being united at the top, and metallically connected at two or more places below, and within 50 feet from the apex of the spire. Such a system of conductors might be so disposed as to become even an ornament to the spire; and for protection against lightning I am not aware of a better. Should the spire be surmounted by a cross or any other metallic ornament, the system of lightning conductors ought, of course, to be united with it. Metallic points and sharp edges have great influence, and must always have a superiority over blunt or rounded terminations at the upper extremity of any system of conductors whatever.

W. STURGEON.

Royal Victoria Gallery of Practical Science,  
Manchester.

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*Liverpool, Sept. 22, 1841.*

Dear Sir,—I yesterday received a letter from the secretary of the Liverpool Polytechnic Society, requesting that I would furnish you, *by return of post*, with the account of the appearance of St. Martin's Church in this town, which suffered severely from a discharge of the electric fluid on the morning of the 24th of August. I have endeavoured to comply with his request by throwing into form the

substance of some observations which I had the pleasure of laying before the Polytechnic Society at the last meeting, on the 24th ult. I am sorry the shortness of the notice has not enabled me to present the information in a form more suitable to the pages of a scientific journal, but trust that it will, at least, be a sufficient excuse for not having done so.—I am, Sir, yours obdly.,

CHARLES J. SMITH.

To W. Sturgeon, Esq., Editor of the Annals of Electricity.

On the night of the 24th and morning of the 25th of August, a very violent thunderstorm was experienced in Liverpool and the neighbourhood, and the churches of St. Michael's, in Pitt-street, and St. Martins-in-the-Fields, were severely injured by the electric fluid.

The latter church, to which these observations are confined, lies nearly north by west (*not north-west*) from the former, at a distance of a few yards more than a mile and a half in a straight line, which line would be nearly parallel with the course of the river, and rather more than half a mile from its banks. At the distance of about 300 yards from St. Martin's, between the church and the river, runs the Leeds and Liverpool Canal, on both banks of which, and in the immediate neighbourhood of the church, stand a large cotton factory, gas works, and several chemical works, having chimneys nearly as high, and in one instance (Mr. Muspratt's) higher than the spire of the church, but none of these were at all affected.

The church, which is in the Gothic style of architecture, was consecrated in January, 1829, and is built of a freestone so strongly impregnated with iron, or some other mineral substance, that it has become perfectly black by the action of the atmosphere. The tower is 126 feet high, and is surmounted by an octagonal spire 94 feet high, making a total altitude of 220 feet. Through the apex of the spire, which is finished by a stone pineal, runs a rod of iron an inch and a quarter in diameter, and 24 feet long. The upper extremity of this rod has a nut upon it, which is sunk, and leaded into a hollow on the top of the pineal, and is covered with cement, so that there is no metal exposed. Two feet lower is another large nut, and a plate resting upon the apex of the pyramid, and the lower extremity runs through the centre of an octagonal block of stone, to which it is secured by a large plate of iron and a nut on the underside. The spire from the top to this stone is almost solid, and thence to the tower it is formed by a wall 9 inches thick, and in 16-inch courses. Each stone is attached by strong copper cramps to the adjoining one on each side, and by copper dowels to the stone above and below. These, with the lead runnings, which have in many places spread considerably between the joints, form, as it were, a metallic chain between each course of masonry. The lightning has passed down the iron rod, the lower extremity of which is covered with a thick laminated coating of oxide, without leaving any trace of its passage. A little below this, however, the destructive effects of the fluid



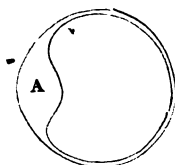
become frightfully visible; the interior of the spire appears as if it had suffered from an explosion of gunpowder. The vertical joints of the stone work are opened, and the edges broken off, as if wedges had been driven between them, and an immense mass of masonry, on the east side, extending to six courses deep and about one-third the circumference of the spire, has been driven completely out, the stones, broken into fragments, covering the roof of the church, and being strewed over the church-yard to an immense distance.

The copper dowels and cramps are broken short off, and one of the latter, on the west side of the breach, bears evident marks of partial fusion. The broken end appears converted into a spongy semi-metallic substance, very friable, and covered with a sort of bloom, interspersed with small spots of a dull red and greenish colour. On the opposite side the spire there is a vertical crack 5 or 6 feet long, sufficiently open to permit the observer to see through.

The electric fluid has not discharged itself by the large breach on the east side, but has passed down the interior of the spire, chipping the vertical edges of the stones in many places, while the horizontal edges seem quite undisturbed. These appearances are not confined to any one side; indeed it appears as if the body of electric fluid had been so vast as entirely to fill the cavity of the spire. Proceeding downwards, it appears to have escaped through the first, or lowest, sound window, on the north side. This window is a pointed arch, ornamented with mouldings, crockets, and other decorations. The phineal is torn off, as well as several of the crockets on the west side of the arch. The corbel on the same side, which has been cut out of a separate stone, and fixed to the face of the masonry by two metal cramps, has been detached, and the face of the under stone hollowed out like a basin; while the cusps and inner edges of the window arch are shattered and broken off, as if by the blows of a hammer, or by thrusting some large and hard body through it.

The fluid seems now to have passed down the outside of the spire till it reached the basement course, which projects about an inch, both inside and outside, all round. Here it has again pierced the wall, and made its way back into the interior. The roof of the tower, or, as it may be called, the floor of the spire, is covered with lead, and a piece of sheet lead is fixed all round the inside, between the projecting basement course and the one above it, and bent downwards towards the floor, which, however, it does not reach by several inches. The fluid from the outside has penetrated beneath the edge of the lead, and through it about an inch below the edge of the stone, making an irregular round hole about an inch in diameter. The edges of the hole are very thin and ragged, being forced outward from the stone, and torn in a radiated form, like the cracks round a hole in a pane of glass. From this spot the fluid has crossed the lead covering of the floor to a door on the east side, and has passed over the sill and down a three-inch lead rain pipe, the mouth of which lies immediately under the sill, in the centre of

the east side. The course of the fluid may now be traced without difficulty, as it has passed along the pipe, which is curved into the south-east corner of the tower, and passes down through the bell loft and ringers' room, and thence through a glazed window on the south side of the tower, immediately above the roof of the church. This pipe is formed of many lengths, the lower end of each entering a little way into the upper end of the next below it, without being soldered. Each of these joints bears curious and interesting evidence of the passage of the fluid, as may be easily understood by reference to the annexed simple diagram, representing a horizontal section of the two pipes at a joint. By this it will be seen that the joint appears as if a round instrument had been thrust in at A, and



forced down between the pipes forming an opening about the size of a tennis ball. The edges of the lead are in some places stretched; and are melted and discoloured as if by the action of a flame applied by the blow-pipe. The glass in the window through which the pipe passes is in small panes, fixed in a metal sash, and secured tight round the pipe by putty or cement, covered by a very thick coating of paint. This has been torn and peeled off; and the glass is completely shattered to the distance of several panes round the pipe. This leaden pipe now passes a little way over the roof of the church, and discharges itself into the head of a cast-iron one, which runs down the side of the building into the drain. Being unable to get on to the roof of the church, or into any part of the interior except the tower and spire, I could not extend my observations any further; but I have been informed, by one of the wardens, of a circumstance which (if I understand it rightly) will form an interesting subject for inquiry to parties engaged in electrical research.

He states, that at the junction of the lead and iron pipes, instead of continuing its course directly to the ground, the electric fluid has penetrated the wall to a gas pipe which runs along the interior, having forced its way through some brickwork in its passage. That the electric fluid should leave its uninterrupted course along the pipes, to force its way through the imperfectly conducting materials of which the wall is composed, seems to be wholly unaccountable, and contrary to the known laws of electricity.

I hope shortly, however, to have an opportunity of examining into the circumstance, and shall be happy if the result of an examination may enable me to explain what at present seems quite inexplicable.

A little after nine o'clock on Tuesday morning, September 14th, a most tremendous crash of thunder was heard at Manchester, but afterwards little or none was heard till towards the evening, when the lightning became almost one continued blaze, and distant peals of thunder rolled nearly the whole of the night. About the time that the first thunder was heard at Manchester, a flash of lightning struck a house at Fairfield, a small town about five miles distant, the particulars of which are described in the following letter :—

*Fairfield, September 16.*

SIR,—Having observed that you are collecting accounts of electric storms, I judged that some notice of that on the 14th of this month might be not unacceptable to you. The weather of the two preceding days had been extremely hot, and at six o'clock on the morning of the 14th, repeated but distant peals of thunder were heard; about half past nine, however, the electric fluid struck a house near Mr. Buckley's vitriol works, and descending the chimney, broke the windows, detaching likewise the wooden framework from the walls. A woman, who was at that moment descending the stairs, was struck down, but escaped uninjured; a girl, who was kneeling at the fire-place, remained for some time senseless, but is now recovering. The handle of an umbrella was broken off, and the stock split to the bottom, and a table shared the same fate.

Perhaps you, sir, or some of your correspondents, may also have remarked, that when a system of cirri, or any other cloud arranged in lines, appears on the heavens, they are almost invariably drawn up parallel to the magnetic meridian, or nearly so. I was much struck with this fact lately, but am at a loss to account for it.\*

I remain, Sir,  
Your very obedient servant,

W. SLATER.

To the Editor of the *Annals of Electricity*, &c.

The electric storm of the 14th September traversed Cumberland, Westmoreland, Durham, Yorkshire, Lancashire, Cheshire, and Derbyshire, at all of which places the lightning and thunder were most terrific; and it is highly probable that it visited many other parts of England. In Yorkshire and Lancashire the night was splendidly illuminated by the rapid succession of lightning discharges.

\* We have frequently observed this fact, and more particularly and correctly do these clouds become arranged according to the magnetic meridian, on the day previous to an evening's display of the Aurora Borealis, and we have no doubt whatever of their being closely connected with the cause of that phenomenon. We stated, at the meeting of the British Association, at Liverpool, that such was our opinion, and also that these clouds, and other much more attenuated sheets of vapour, modified considerably the display of the aurora on many occasions; and we should not hesitate to venture an opinion, that the illumination of these narrow sheets of vapour, in many cases otherwise perfectly invisible, constitutes the streamers of the aurora, even on those occasions where no cloud appears to be present; for, notwithstanding the brightness of the southern heavens, and the brilliancy of its stars, the northern heavens are usually hazy during the display of the aurora.—EDIT.

At Lymm, in Cheshire, about eleven o'clock at night, the lightning fell in a garden belonging to J. Leaf, Esq., and the crash of thunder that attended it terrified the whole neighbourhood for several miles round. The noise was tremendous even six miles distant. Thinking that considerable damage must have been done to some part of the premises by the effect of such a formidable discharge of lightning, Mr. Leaf and some of his family were induced to go out and make the necessary examination. The buildings, however, had sustained no damage, but in the garden, in the rear of the house, a large hole had been broken in to the depth of about a yard and a half.

About a week after this occurred, I was told of the circumstance by the Rev. Mr. Johnston, who thought it possible that some drain might have been the destination of the lightning, and that it had broken into it, and the soil fallen down as a matter of course; or it was possible that a meteorite might have fallen. These opinions appearing to be well founded, and the parties wishing me to visit the spot, I repaired thither, about fifteen miles from Manchester, as soon as I could make it convenient. The hole was partly in a pea-bed and partly in a potatoe-bed, its centre being directly between them.

I first got Mr. Leaf's servant to clear away the sunken soil from the bottom of the hole; which done, and deepened to nearly two yards and a half, he found a nearly horizontal cavity in the substratum of sand. The hole sloped a little downwards, and was large enough for the man to creep into, but it proceeded only about a yard and a half in that direction. The Rev. Mr. Hutchinson, who is next door neighbour to Mr. Leaf, sent his servant man to our assistance, and by the joint labours of the two men, a great quantity of earth was soon cleared away; and although no meteorite was found, there were sufficient indications of a formidable power being at work. Broken pea-sticks, pea-straw, split potatoes, and soil from the surface, were found at various depths, as low down as five yards; and in several places the soil which was forced down, which is quite mellow on the surface, was compressed into a state of compactness equal to that of stiff brick clay. Several small stones, also, such as whin-stones, being found split into fragments, and exhibiting recent fractures, and these at different depths, indicated that a violent force had been exerted. The probability is, that a discharge of lightning had struck that place, and that, from some cause or another, a cavity had previously existed in the sandy substratum, into which the upper stratum was driven by the electric force. No marks of the effects of a very high temperature were observable amongst the inflammable materials surrounding the hole.

W. S.

## ATMOSPHERIC AND ELECTRICAL PHENOMENA.

On Tuesday night the 14th of September, from half-past ten to half-past eleven o'clock, there was, in Manchester and the immediate neighbourhood, a rapid, we had almost said constant, succession of lightning flashes, which illuminated the whole of the heavens; and, the night being otherwise dark, the effect was very striking. As no thunder was heard, this lightning (vulgarly called ground or "summer lightning," and erroneously believed to be unaccompanied by thunder) must have had its origin at some distance; but at times it seemed to fill the atmosphere with its vivid light. It appeared at first to be nearly due W. but it gradually became N.W. and ultimately appeared to be due N.; so that in all probability the progress of the storm, of which this lightning was the indication, would be from W.S.W. to N.N.E. On Wednesday evening, about a quarter before six, the clouds, which had been louring during the afternoon, became partially broken, and there were one or two showers of rain. The sun was shining with great brilliancy, and almost immediately after the shower commenced, a very splendid and perfect rainbow was formed, which throughout was remarkable no less for the extreme beauty and vividness of its colours, each band of which was distinctly visible, than for its uniform brightness and distinctness. The sun being near the horizon, the arc described was of considerable apparent extent, and was visible, without any apparent diminution of its brilliancy, for at least ten minutes. It was accompanied by a second less bright external bow, which, however, had more distinctness than many primary ones that we have seen; the two being further apart than is usual. When the second bow disappeared, the other gradually melted away from its fine, well-defined form, the southern limb disappearing first, and the northern limb, near the horizon, remaining for some minutes afterwards as distinct as at first. At one moment, as if the sun had poured a full tide of his beams upon the aerial spectrum, this portion of the bow near the horizon shone out with surpassing effulgence, the effect being that of a transparency receiving a large accession of light behind; the prismatic colours were more plainly separable than before, this departing glory apparently illuminating a circular space of the Iris.—*Manchester Guardian*.

## DESTRUCTIVE THUNDER STORM AT ADDINGHAM.

On Wednesday night, Oct. 20th, between six and seven o'clock, the village of Addingham was visited by an awful thunder storm, accompanied by a heavy fall of hail, mingled with pieces of ice. The house of H. A. Bramley, Esq. was much 'damaged by the electric fluid. One of the chimneys was shattered to pieces, the stones being hurled in every direction. The slates were much damaged, and the lead on the roof was melted. The lightning descended into almost every room, shattering the glass and frames

of most of the windows, and doing considerable damage to several articles of ornament, The curtains of the drawing-room caught fire, and were destroyed, and the plastering of the walls and ceiling were more or less injured in every apartment. Fortunately none of the inmates received any injury, further than being dreadfully alarmed. Two servants in the kitchen were lifted off their feet, and Mrs. Bramley had a candlestick struck out of her hand. A man on horseback in the street was, with the horse, knocked down.

## MISCELLANEOUS.

### LETTER ADDRESSED TO THE EDITOR.

12th August, 1841.

SIR,—Having for some years past been earnestly engaged in the study of Electricity and Chemistry as far as my limited time would allow, I have repeatedly remarked various circumstances of coincidence in the properties of the supposed chemical elements, which have long since led me to believe that many of those we now class as such, are, in reality, compounds, particularly those whose atomic weight is very high, possessing at the same time properties very similar to others whose atomic weight is lower; iodine, for instance, bearing many points of resemblance to bromine, the atomic weight of which is less than two-thirds of the former. Again, both of these bodies bearing a still greater analogy to chlorine, whose atomic weight is less than one-half that of bromine, and one-third that of iodine. Such being my own private view of the subject, on inspecting your current number for August, I observe with peculiar satisfaction the notice of Dr. Samuel Brown's labours in connection with this subject, especially as it appears to prove, almost indisputably, the intimate relationship of carbon to silicon. In addition to the proofs he advances in support of his hypothesis, I feel happy to contribute a curious experiment of Vauquelin, which appears in some measure to verify it. It is as follows:—

“The excrements of a hen fed for ten days on oats, on being calcined and analysed by the above chemist, produced twice as much carbonate and phosphate of lime as was originally contained in the oats, while a deficiency of silica was observed, which might have been employed in furnishing the excess of calcareous matter, a transmutation depending upon the absorption of some unknown principle amounting to five times its weight.” This extract from my note book is copied from Richeraud's *Physiology*, p. 259, Copland's Translation, I believe; but as it is some time since I read it, I cannot give the edition. I have examined the translation by De Lys, which I have in my possession, but it is there omitted. By substituting the word carboniferous instead of calcareous in the above extract, a change it will readily bear, this extract, I think,

affords additional proof of the isomeric nature of carbon and silicon. Should this prove worthy a place in your valuable Annals, I hope, at some future time, to contribute a more precise statement of my views on the subject, when I have had sufficient time to arrange them in proper order.

I am, Sir, yours respectfully, W. H. TOWNER.  
London, 3, Rutland-street, Sydney Square, Mile End.

#### CASE OF POISONING BY LAUDANUM.

A very interesting case of poisoning is reported from the Middlesex Hospital in the *Lancet*. A man was admitted a short time since into that hospital about six hours after having taken an ounce of laudanum (containing twenty-six grains of opium.) At the time of admission he was apparently lifeless; the surface of the body was cold, countenance pale and livid, lips purple, pupils contracted to a mere point, respiration scarcely perceptible, pulse hardly to be felt. The laudanum was removed by the stomach-pump, but, in spite of every exertion, the pulse became more unfrequent, and was at times imperceptible, when recourse was had to electro-magnetism, which was applied by means of a small battery, with coil and contact-breaker. One wire was applied to the neck, and the other to the region of the heart or epigastrium, and by these a succession of very powerful shocks was given. Its good effects were very apparent. The muscles of respiration were set in action, and the diaphragm contracted powerfully; the chest was more fully expanded, respiration was more perfectly carried on, and a corresponding improvement was observable in the countenance. The pulse improved and became more powerful, becoming steady when the current was interrupted for a few minutes. This application was continued for several hours, and was finally successful—thus clearly establishing the influence of electro-magnetism under circumstances hitherto considered quite hopeless.

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*Note on the Indication of Capillarity of some Liquids.* By M.

L'HERMIT, Licentiate of Sciences, Member of the Pharmaceutic Society of Emulation.\*

The experiments of which capillarity has hitherto been the object have had essentially for their aim to research the laws to which these phenomena are submitted, and to verify the results of calculation. The theory of Laplace, which has been perfected by Poisson, by bringing into consideration the least density of the liquid at its surface, in a layer or film of insensible thickness; this theory, I say, agreeing perfectly with the experiments made by M. Gay Lussac, principally on water and alcohol, we may with all certainty employ the formula to which it conducts, to calculate the capillary force of other liquids.

This study is of some interest, especially at the present day, when organic chemistry presents us with so great a number of liquids, of

\* *Journal de Pharmacie.*

which several are formed of the same elements in the same proportions. In order to establish the difference between them, we shall find in the molecular force a means perhaps more general than the refractive power or the property of impressing on luminous rays the circular polarization; for, at first, all do not possess this latter property. It follows, as a result of the experiments of M. Henri Deville on one part, and of M. M. Edmond Becquerel and Cahours on the other part, that these isomeric liquids of densities almost equal, have indications of refraction of the same value. Thus, following the first-named philosopher, the essences of citron and of turpentine would have exactly the same indication, 1.472, whilst the heights which they attained in the capillary tubes of the same diameter differ remarkably. These are, here I may add, the only isomeric liquids on which I have been able to operate hitherto; but it is perhaps on this species of bodies that it is of the greatest import to experiment.

These experiments, the results of which are detailed in the following table, have been made with the apparatus of M. Gay Lussac. We have valued the ray of the tube employed, by weighing a column of mercury contained in the tube, and the length determined. As it is very difficult to procure tubes of which the diameter shall be the same throughout their whole extent, we, with great care, measured the column when it was placed in such a manner that the middle of its height corresponded nearly to the part where the liquids ought to be arrested in the experiments on capillarity, that is to say, at two or three centimetres from the lower extremity. We know, besides, that the formula to calculate the ray by is—

$$r = \sqrt{\frac{P}{\pi m l}}$$

In designating the  $P$  the weight of the column of mercury, by  $l$  its length, by  $m$  the weight of a millimeter cube of mercury, for the temperature at which we operate, by  $\pi$  the half circumference of which the ray is 1 for my tube  $r = 0^{\text{mm}}, 38928$ .

The low temperature at which these observations have been made is a vexatious circumstance for some of these liquids, which are then more or less viscous. In this case, the friction which the fluid molecules exercise one on the other is an obstacle to the precision of these results. If, after having wet the interior of the tube to a certain extent, by means of the liquid on which we are about to operate, we plunge the extremity of the tube in the same liquid; if it is viscous, it will raise itself to a height a little inferior to that which it will preserve, when we should have made it ascend still higher than it would have risen spontaneously, by plunging the tube, during some seconds, deeper into the liquid, and again putting it in its place. The best way is then to take the mean of the observed heights in the two cases; it is that we have done with the essence of cloves, which, besides, not being extremely pure, from the sweet oil of wine and crystallizable acetic acid.



After Poisson, the height at which the liquid fixes itself in a tube which is wet with it, is expressed by the formula—

$$h = \frac{a^2}{r} - \frac{r}{3} + \frac{r^3}{3a^2} (\log. 4 - 1)$$

$h$  the height above the free surface;  $a^2$  dependent value of the molecular force, and which we name the *index of capillarity*.

When the ray of the tube is very small, the third term of the second member of the equation may be altogether neglected. We have then to calculate the value of the index—

$$a^2 = \left( h + \frac{r}{3} \right) r$$

It is by this formula that we have obtained the numbers of the last column of the following table:—

NAME OF SUBSTANCES.	Temperature.	Means of heights observed in millimeters.	Indexes of capillarity or value of $a^2$ in square millimeters.
Concentrated ammonia of 0.957 density..	3°	35.2	13.753
Concentrated sulphuric acid at 66°.....	— 1°	26.8	10.483
Crystallizable acetic “ .....	10°	14.85	5.8313
Do. “ “ of 1.057 density	— 2° 4	15.1	5.928
Sulpho-venic “ .....	1°	17.165	6.7325
Sulphur of carbon .....	— 0° 4	14.25	5.5978
Naptha .....	— 0° 4	17.3	6.785
Sweet oil of wine, heavy.....	0° 5	17.75	6.96
Chlorine.....	0° 7	16.175	6.308
Acetic ether.....	0° 7	14.475	6.088
Do. “ of which acetic acid odour } announces a partial decomposition.. }	— 0° 4	15.1	5.928
Bi-hydrate of metheglin.....	0° 7	15.5	6.1844
Azotate of “ .....	0° 7	13.9	5.4615
Sulphate of “ .....	0° 7	16.65	6.532
Acetone.....	— 1°	15.95	6.2595
Conecini .....	— 1°	18.1	7.0965
Essence of bitter almonds.....	— 1°	19.475	7.6317
Do. “ menthi.....	— 1°	16.8	6.5904
Do. “ cloves.....	1°	17.5	6.8629
Do. “ citron.....	1°	17.2	6.746
Do. “ turpentine .....	1°	16.7	6.5514
Do. “ mustard .....	1°	17.8	6.9797

M. Gay Lussac has observed, with the essence of turpentine and in a tube with 0<sup>m</sup> 6472 of a ray, an elevation of 9<sup>m</sup> 8516, which gives  $a^2 = 6.5155$ .

## ELEMENTARY LECTURES ON ELECTRICITY, &amp;c.

## LECTURE XIII.

While prosecuting our experiments with the Leyden jar, it may be as well to divide them into certain classes, accordingly with the character of the phenomena to which they give rise ; and we will begin with the mechanical class of phenomena. Indeed, we have already shown some phenomena of this kind, for wherever motion is produced, the phenomena come under the laws of mechanics.

I think it possible, however, that you may think that that which is *electrical* cannot be *mechanical* ; because I am well aware that such notions have been, and are now, entertained by gentlemen of considerable scientific attainments ; and I have even heard this topic strongly contested in discussions between scientific parties. However, I am very far from being the first who entertained the idea that mechanics may have an *electric* origin, nor can I see that mechanics are confined to any peculiar character of force whatever ; for illustrations of the laws of mechanics we more frequently than otherwise employ the force of gravitation, whether we be contemplating solids or liquids. But gaseous bodies present mechanical forces of a very different kind to those of gravitation, that is, *elastic forces*. Solids, also, when in the character of springs, afford elastic forces as well as those due to gravitation. The power of aqueous vapour, or steam, is the most formidable of mechanical forces under the control of man. The deflections of a compass needle, although of magnetic origin, are subject to the laws of mechanics. Heat also produces mechanical action. Why not, then, have electro-mechanic action also.

Mathematicians have a fashion of dividing mathematics into *abstract mathematics* and *mixed mathematics*. By the former term they mean the art of calculating independently of any reference to physical bodies, or physical forces, and by the latter they mean calculations *applied* to real bodies, or to existing forces. Hence, it has often occurred to me, that *applied mathematics* would be a much more appropriate term than *mixed mathematics*. Be that as it may, it is very obvious that the laws of mechanics may be contemplated quite as abstractedly as pure mathematics, and those laws are applicable to all kinds of forces whatever.

Let us now place about a dozen folds of writing paper, one upon another, on the tin foil on which we place the jar when charged, and when the jar is to be discharged I will place the lower ball of the discharging-rod on the top of the paper pile, pressing it down pretty well ; and I then bring the upper ball towards the ball of the jar. You now see the flash of light and hear the report as distinctly as in any

(To be continued in our next Number.)

THE ANNALS  
OF  
ELECTRICITY, MAGNETISM,  
AND  
CHEMISTRY;  
AND  
GUARDIAN OF EXPERIMENTAL SCIENCE.

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DECEMBER, 1841.

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ROYAL VICTORIA GALLERY OF PRACTICAL  
SCIENCE, MANCHESTER.

Dr. PLAYFAIR'S *Lecture on Miasmata.*

There is no science which can be compared to chemistry for the rapid progress which it has made in recent years. Indeed, the science itself is quite modern, but the importance of the objects which are its peculiar study has attracted so many disciples, that it has progressed with a rapidity quite unprecedented in the history of science. That empirical art, whose whole ambition was to enable her cultivators to prepare the nobler metals for the selfish purpose of enriching themselves, is now relieved from the trammels which cramped and blunted all her energies, and has appeared in all her native dignity, as the science whose duty is to examine the laws of nature, and whose domain is so extensive that there is now no science, art, or manufacture, which can dispense with her aid and assistance. Her province is to examine into the nature and composition of all substances which occur in the material world, and to investigate the laws which regulate their formation. Now, matter may be divided into two great classes; first, that which enters into the composition of the inorganic kingdom; and, second, that which forms the organic. By inorganic substances, we mean the minerals and rocks which occur in nature, and the products arising from their disintegration or decomposition. Although the forms in which matter is presented to our view are so varied and numerous, yet chemists have not been able to detect more than 54 simple substances, which, by admirable adjustment and combination, form all the variations of matter that are found on the globe. The greater number of these elementary substances occur in the inorganic kingdom.

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But organic matter is very differently composed. It consists of a very limited number of substances, and it is only the different arrangement of these which produces all the varieties of form and structure which occur in the vegetable and animal kingdoms. You are aware what these substances are; they are only four in number, and have received the names of carbon, oxygen, nitrogen, and hydrogen. The first of these, carbon, is familiar to you all under the term charcoal or diamond, which is carbon in its purest form. Oxygen and nitrogen are the two substances which form our atmosphere, but they are quite opposed to each other in properties. It is perfectly necessary to remember the two characters of these, before a clear idea can be formed of the points in animal chemistry, which will be presented to your attention.

Oxygen is a body possessed of a wonderful range of affinity; it possesses in an eminent degree the power of attacking a great variety of substances, and of forcing them to enter into combination with it. Many examples of this are familiar to you all. When a piece of iron is exposed to the air, it becomes covered with rust. Now this rust is merely a combination of the iron with oxygen; and it is in virtue of this disposition of oxygen to enter into combination, that the iron has become oxydized or rusted.

Now, nitrogen, the other constituent of the atmosphere, is quite opposed in character to oxygen. The principal characteristic is an extreme indifference to every other body, and an apparent reluctance to enter into combination with them. Indeed, it might be considered to be the chemical bachelor, who cannot be persuaded voluntarily to enter into union, and when compelled to do so, it merely exists in it, by a kind of *vis inertiae*, as if it were too idle to dissolve it; but the moment it is assisted by any exterior cause, it escapes in a single state, apparently with the utmost satisfaction.

The only remaining substance which enters into the composition of organic bodies, is a gas called hydrogen. It shews a great disposition to unite with oxygen, and the substance formed by the combination of these two bodies is water.

These preliminary remarks have been necessary, in order to enable you to understand the condensed view which I shall now present of the recent discoveries made by Liebig in explaining the processes of contagion and miasmata.

I shall not, therefore, confine myself exclusively to the subject of contagion and miasmata; but describe also some interesting topics connected with the nutrition and respiration of animals, according to the views of Liebig. In nature, we observe a beautiful link connecting the different kingdoms. Vegetables obtain their nourishment from inorganic substances. The herbivorous animals are supported by these vegetables, and they in turn afford food to the carnivorous. Hence, it is evident that vegetables must contain matter adapted to the nutrition of animals. What is this matter? There is a substance in all animals called *protein*, from a Greek word, signifying

“the foundation of.” This protein forms the basis of every part of the animal economy. It is a constituent of blood, of muscular fibre, of the tendons and ligaments—in short, of every part of the body. Now, this substance called protein is precisely of the same composition, in 100 parts, as albumen or fibrin. Albumen, you are aware, is the white of egg, and fibrin is a substance procured from blood. In all vegetable substances, capable of yielding nutrition to animals, nitrogen is a constituent, and this nitrogen is always present as gluten or vegetable albumen. This gluten and vegetable albumen has also the same composition, in 100 parts, as protein. Indeed, albumen, fibrin, gluten, and vegetable albumen, may be considered as protein in different degrees of condensation. We therefore see that vegetables contain the basis of the animal organization, and that by the assimilation of this matter, the nutrition of herbivorous animals may be effected. As far as regards carnivorous animals, there is no difficulty; for the flesh which we eat is precisely of the same composition as that contained in our own body; so that in eating the flesh of other animals, it is virtually as if we eat ourselves. If there was no waste in our bodies, there would be no need of taking nourishment; but parts of our bodies are lost every moment, so that it is necessary to replace these lost parts. The food, which is capable of thus replacing what is lost, must necessarily contain substances of the same composition, and these I have mentioned are modifications of this wonderful substance, protein. Of course, there are other substances necessary for the formation of other parts of the body, such as the bones; and the inorganic matter requisite for this purpose must also be contained in the food. The amount of food taken in the normal state should be as much and no more than is sufficient to replace the lost parts. But the quantity will vary with the kind of food; for some kinds of food contain more of this protein than others. Thus, rice contains very little nitrogen, and consequently we find that the natives of India eat enormous quantities of this grain, quantities which astound a European. But the reason is, they must eat sufficient to make up for the loss of nitrogen abstracted from the body. It is a very remarkable illustration of this view, that at certain periods of the year, when such an amount of food would be prejudicial to them, they drink the juice of the palm tree. Mr. Ransome has stated, in his lectures to you, that the juice of trees contains ammonia, and ammonia is a substance very rich in nitrogen, so that the natives become supplied with a sufficiency of this indispensable element. If the position of Liebig be true, namely, that it is only those matters possessing a similar composition to the animal tissues which are capable of assimilation, it is natural to inquire the use of the other substances contained in the food, such as sugar, starch, and gum.

The Doctor then entered into this inquiry, and into the peculiar action of animal and vegetable substances on the human constitution, and the effect of various descriptions of food which are

used in different climates; after which he proceeded:—Such, then, are some of the processes which occur in the frame of man during life, when that wonderful principle, vitality, opposes itself to the action of the chemical forces. Indeed, the whole life of man is taken up in the conflict of these antagonist powers, and the results of this conflict vary according as the vitality or the chemical forces gain the ascendancy. But when the mysterious principle of life departs from our weak frames, then the chemical forces reign triumphant, and man returns to the dust from whence he came. The elements which, during life, he held almost at command, now take ample revenge after death. The process of the decay of animal and vegetable matter is extremely interesting, and explain to us in a simple manner the action of contagion and miasmata.

When we place a piece of decayed wood on a piece of wood perfectly sound, we observe, that after a short interval the sound wood becomes affected, and enters into the same state of decay. Here, then, we observe that the decayed wood has imparted or communicated its state of decay to the wood in its vicinity, just as a piece of burning wood imparts its combustion to another piece placed in contact with it. You must have observed the same fact, when fruit is preserved in the same place, that if one becomes decayed, the decay is speedily propagated through the whole mass. Now, this is contagion in vegetable matter strictly analogous to that which proceeds in the animal economy. You have all heard of the dangerous and even fatal consequences which ensue, when the anatomist cuts himself with the knife which has been employed in the dissection of a carcase. And the reason is, that the state in which the dead body is, is conveyed to his own system. There is a disease, happily for us unknown in England, but which is of frequent occurrence in Germany, that illustrates this view very completely. It is occasioned by eating bad sausages. In Wurtemberg these sausages are made in an extremely bad manner, and hundreds of cases of this disease have happened there. Blood, liver, bacon, brains, meal, and bread, are mixed with a few spices, made into sausages, and, after being boiled, are smoked. These sausages are very apt to become bad, and if they are taken in this state, they produce a lingering disease. The patient becomes quite thin, and loses muscular fibre, the fluids in his body dry up, and he presents exactly the appearance of a mummy—finally he dies under excruciating suffering. It is very remarkable that his body, after death, is not subject to decay. The cause of all this is obvious; the state of decay in which the sausages were, having been communicated to his system.

Some time since, a party in Switzerland, if I remember rightly to the number of ten, partook of some bacon, which was very far gone; they were all taken alarmingly ill, except four, and those who became ill died in consequence. Upon inquiry, it was found that the survivors had all partaken of strong coffee immediately after their meal, which the others had not. Now, coffee is well known to be an

anti-putrescent substance, and it was by virtue of this property that these individuals were saved.

Those who are acquainted with the process of brewing beer, are aware, that by the addition of a small quantity of yeast to a saccharine vegetable juice, the whole is thrown into a state of fermentation; but the yeast itself must be previously in fermentation, otherwise it will not act. What I wish to draw your attention to is, that during the fermentation a large quantity of yeast is formed. Now here the yeast appears to reproduce itself, but it is not so, for the new yeast is produced from the gluten contained in the juice. We find, in a great number of chemical phenomena, in which a body communicates its own state to another in its vicinity, that the first body is reproduced, especially if the substance from which it was originally formed be present. Thus it is with yeast, for vegetable juices contain gluten, the substance from which yeast was originally formed.

When the fermentation of the beer is conducted at low temperatures, a peculiar yeast is formed, which sinks to the bottom of the liquid; but if the fermentation be conducted at higher temperatures, a light yeast is produced, which rises to the surface. These two kinds are quite different in their mode of action, for the one produces a violent tumultuous fermentation, whilst that occasioned by the other is tranquil and slow.

How beautifully analogous to this are certain kinds of contagious matter, such as that of the small-pox. You are aware there are two kinds of this disease, one called the human small-pox, the other the cow-pox. The virus of one produces a violent dangerous disease, whilst that arising from the action of the other is mild and gentle. The blood contains different substances at different stages of our growth, and thus it is that we are liable to peculiar diseases in different ages. The blood of children contains a substance very apt to be thrown into a peculiar state of decomposition by the contagious matter of scarlet fever. When they have received the contagion, the disease proceeds until all this substance is destroyed, after which they are not again subject to it, except in a few cases in which those substances susceptible of contagion are again formed. It is this re-formation, in the case of the matter susceptible to the contagion of small-pox, which renders reiterated vaccinations necessary.

From what I have said regarding the yeast, it is evident that the contagious matter, which had its origin in the blood, will again be produced in much larger quantity during the disease. Hence it is that contagion, when it once appears in a district, spreads itself with such rapidity.

Hitherto I have spoken only of contagious matter in a solid state, but the remarks made on this subject are also applicable to gaseous contagions and miasms. The difference between contagion and miasm is this. *Gaseous contagious* matter is generated from the blood, and capable of reproducing itself in the blood. *Miasm*, on the contrary, is a product of the decay of animal or vegetable matter,

and acts upon the blood without being capable of being reproduced. Those substances are also called miasms which act by chemical means, not by the state in which they are.

Gaseous contagious matter is always in a state of decay. This was proved by your late illustrious townsman, Mr. Henry. He placed vessels, filled with ice, in rooms containing contagion. The aqueous vapour of the apartment condensed on the outer surface of these vessels, and brought down the contagious matter with it in a state of solution. Now, this matter exhibits all the characters of a body in a state of decay. All the substances employed for the purpose of destroying contagion are merely those which are anti-putrescent, such as the burning of tar, chlorine, &c. Indeed, the remedies often given are unwittingly for the same purpose. Thus, in Egypt a new remedy is now very extensively adopted for the plague. This is, to force the patient afflicted with the disease to drink as much brandy as he can be possibly made to take. Now, brandy and alcoholic liquids in general, are powerful antiseptic means, and destroy the species of decay proceeding in the body. It must be remembered, that it is only on particular accidental ingredients of the blood that these gaseous contagious matters have influence. These ingredients may be formed either by the use of unwholesome food, by intemperate habits, or frequently even when all circumstances are favourable to health. Their presence is entirely accidental, and not in any degree necessary for the maintenance of the vital functions. We do not know how to guard against their formation, but further investigation may enable us to determine this important point. This power of imparting a pre-existing state to another body is no hypothetical force; it is a chemical power, which exerts much influence in the phenomena of combination and decomposition. Nor is it hypothetical that the acting matter is capable of being reproduced by the body on which it acts, for we have seen that yeast causes its own reproduction. Now, we can explain how and why it is that certain people escape from certain contagion, whilst others are affected by it. It is because their blood does not contain the materials capable of being thrown into the same state of decomposition as the matter of contagion. Nor do we find any difficulty in assigning the cause for the propagation of contagion, for the contagion is reproduced. As to the properties of contagious matter in general, I think we may consider that it is possessed of a considerable specific gravity. Prout found, that when the cholera visited England, the specific gravity of the air was greater, during its stay, than it is usually. The contagion of typhus fever not only appears to have a considerable specific gravity, but also to be in such a rapid state of decomposition, that its noxious property appears to be very speedily destroyed. Some time since, in the Infirmary at Edinburgh, the surgical wards were so crowded that the patients were removed into the fever wards. The room was long, and on one side the fever cases were kept, the surgical cases on the other.



Now it is very remarkable that not one of the patients on the opposite side of the apartment received the contagion, whilst the students, who had to lean over the beds of those afflicted with the fever, were severely affected with it. Here the contagion did not seem capable of passing over the short space which intervened.

I have mentioned to you that miasm differs from contagion by being the product of the decay of animal and vegetable matter. You are all aware of the horrible diseases which it produces. In many cases the action of miasms is purely chemical. Mr. Daniell has lately shown that some parts of the African coast are covered with that noxious gas called sulphuretted hydrogen. This is the gas which occasions the smell of a bad egg, and blackens any silver spoon which may be placed in it. Sulphuretted hydrogen is peculiarly fatal to animal life, by its chemical properties; it combines with the iron contained in the blood, and thus removes an essential ingredient. The presence of this gas on the African coast may well explain its unhealthy qualities. This sulphuretted hydrogen may be considered as one of the most common and pernicious miasms. It is evolved in our own country from wells, mines, sewers, and places used for burying the dead. When in the air, it is best destroyed by burning sulphur in the air. Sulphurous acid is thus formed, which decomposes it.

Carbonic acid is another frequent cause of miasm, and is formed under a variety of circumstances, and its detrimental effects may best be prevented by scattering burnt lime over the place containing it. The miasms formed by the decomposition of organic matter are rather more difficult to explain. In most cases they seem to act like contagious matter, by imparting their own state of decomposition to the blood. They differ, however, very essentially from the latter; they are not reproduced in the blood, because they have not been originally formed from it.

Miasms are generally formed in greatest abundance in warm climates, where sun and moisture accelerate the decomposition of organic matter; but it is by no means confined to these countries, but is, unfortunately, too common in our own, and will continue to be so until that barbarous custom of having our churchyards within our towns—a disgrace to modern England—shall be abandoned, and until a more effectual means be introduced by which better drainage and cleanliness be afforded to our towns.

I have thus endeavoured to give you a slight sketch, a very slight one indeed, of some interesting points connected with Liebig's discoveries in Physiology; but, in justice to my subject, I must mention, that I was drawn to Manchester only a very few days since by accidental circumstances, and without the slightest anticipation of having the honour of addressing you. I trust this fact will be sufficient to cause you to grant me your indulgence for any inelegance of diction or want of perspicuity, which, I am afraid, you must have observed in my paper.

*Report on an Indication of the Level of the Water for Steam Boilers.*  
Presented by M. DAILLOT.\*

M. Daillot, inspector of steam-boats, presented, for the examination of the Academy, an apparatus indicative of the level of the water in steam boilers.

You are acquainted with the fact, gentlemen, that the Administration has endeavoured to make all passengers in our steam-boats participate in the inspection of the engines. It is in pursuance of this unique end that rules are prescribed for the placing on the deck of the vessel, in such a manner as to be perfectly accessible in all respects, monometers which indicate the pressure, and safety valves which limit the tension of the steam.

Desirous of seconding the views of the authorities, M. Daillot thought that it would associate usefully with his just solicitude, if he could submit to the general control on the deck of the vessel the state of the level of the water in the boiler. These indications are in general only noticed by the firemen or mechanics in the engine-room.

It is this desire which has led to the simple but ingenious mechanical arrangement which he submits to you to-day. M. Daillot has taken care to study it, and to prove its useful effects, by a practical application sufficiently prolonged, on several steam-boats on the high Seine.

The indicator apparatus of M. Daillot consists of a hollow column of metal fixed on the boiler. The inferior extremity of this column is in the water when the liquid is in sufficient quantity in the boiler; in the contrary case, its inferior orifice opens into the steam. A cylinder of glass continues and terminates above the column which we have just described. A hollow ball, lighter than the volume of water which it displaces, floats in the liquid of which the column is full. As soon as the base is plunged in the water, this ball indicates, by its position, whether there is sufficient water in the boiler. At the moment when the level falls, the water is replaced in the column by steam, and likewise in the cylinder of glass which terminates it. The ball is then no longer carried towards the superior extremity; it falls, and remains at the bottom of the cylinder. It is in this manner that it gives warning of any intervening change in the state of things.

A tablet, bearing two arrows opposite to each other, at one of which is written the word *safety*, whilst the word *danger* will be written in large characters at the end of the other, could be placed behind the cylinder of glass which contains the indicating ball.

The position of the ball, visible to all, would powerfully provoke, in the case of its depression, the attention of the parties interested

\* Comptes Rendu, 1 Mars, 1841.

to the common safety. The danger would thus be made known to all; it might then be immediately counteracted by the workmen and conductors of the machine. A certain exposure of their negligence in not keeping up the level of the water, would induce, on their part, a more vigilant attention; and if it is necessary to stimulate the love of duty by pecuniary interest, a fine might be made the consequence of the depression of the indicating ball.

One of your commissioners has seen the apparatus of M. Daillot in action, and feels assured of its useful effects. We think, then, that this indicator, of which the construction is still susceptible of some slight modifications, in order to render it more simple and less expensive, is conceived and executed on a rational principle. We propose to you, in consequence, to declare, that, as an apparatus indicating to the eye the state of the level of water in a boiler, it merits your approbation.

The conclusions of this Report are adopted.

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*On the Causes of the Differences which are observed between the Absorbing Powers of Metallic Plates polished or scratched, and on their Applications to the Perfecting of Calorific Reflectors.*  
By M. M. MELLONI.\*

In the last memoir which I had the honour to read before the Academy, there were often inquiries made for plates whose temperature increased gradually by virtue of their absorbing power on calorific radiations. On this subject I have not failed to observe, that the metals, and other substances susceptible of becoming smooth and shining, are constantly found prepared in such a manner as to present surfaces rough and completely unpolished. This was an essential condition of our experiments; for it was necessary to separate the diffusion of the reflection, properly called, in order to pursue, with some chance of success, the object that we had in view. If in place of plates always unpolished and rugged, we employ surfaces sometimes polished and sometimes unpolished, the compared calorific absorptions of plates present results which are very curious, and which we shall now briefly explain.

A disc of brass, whose surface is rough and granulous, becomes heated more under the action of a calorific radiation than a well polished disc of the same substance. Again, a metallic vase, with a rugged surface, full of cold water, cools more promptly than a vase of burnished metal. These experiments have induced a great number of philosophers to admit that the small points or asperities on the superficies of the body augment the absorbing and emitting powers. I have already tried to demonstrate, in a note communi-

\* Comptes Rendu.

cated to the Academy,\* that the emissive power of bodies does not depend on the degree of polish or of roughness which their surfaces have had communicated to them. We shall now see that the same thing takes place with regard to their absorbing power. But before engaging ourselves with the experimental proofs, it is essential not to be mistaken as to the sense of what has just been advanced. Our proposition does not affect the fact itself—that we do not contest—but only the explanation which has hitherto been given of it. Thus, in removing, by friction, and emery or lime, the polish of a metallic body, in such a manner as to render the surface dull and tarnished, from a previous smooth and brilliant state, we alter very certainly the proportion of heat which this body, exposed to the calorific radiation, absorbs in a given time. The alteration may perhaps be so much as to render the heat of the metal double or triple that which it at first possessed; but, however, we maintain that the roughness or the polish has no part in the production of the phenomenon, and that the change is brought about on the absorption of the metallic surface, derived from some other cause. I will now give some experiments to prove it.

When we dispose, successively, before a good thermoscope, a small disc of copper, scratched or unpolished, and a disc that is bright and polished, blacken both of them on the side nearest to the thermoscope, and now direct on their anterior faces the same calorific radiation concentrated by a lens of crystallized salt, we observe what we have above described, that the heat of the scratched disc is superior to that of the polished disc. The same thing occurs if we operate on polished and unpolished discs, either of steel, brass, silver, gold, or of any other metal, reduced to a lamelated state by the action of the mallet or of the flattener. But if we repeat the experiment on two plates of tin, the one of which had been much beaten with slight blows of a mallet, and the other left in its natural state, the heating of that which possessed a plain and mirror-like surface always carried away a good deal of heat on the heating of the first, whose surface is less bright, and is covered with crumples or dinges. Further, if we take two plates of silver or gold, cast, and afterwards slowly cooled, the one of which is possessed of a fine polish, which we can impart to it by means of oil and the carbon from live coals, whilst the other, polished at first in the same manner, is afterwards deprived of its polish by means of a series of rays traced on it with the diamond, we see with surprise that precisely the contrary happens from what ordinarily takes place; that is to say, that the scratched plate is heated less than the bright and polished plate.†

\* *Comptes Rendus de Séances de l'Académie des Sciences, Année 1838.*

† Gold or silver is indispensable, because, if we employ copper, or any other oxydable metal, the scratched surface becomes covered more promptly than the other with a film of the oxyde, which will augment considerably the absorbing power, and will scarcely permit us to distinguish that which belongs to the compared influences of the polished or scratched surface. For the same reason, it will

But if, in removing the polish, we can sometimes augment and sometimes diminish the absorbing power, it is clear that the variation produced is not derived, as is generally supposed, from the formation of points or asperities, thereby introducing a greater quantity of heat, but rather from the changes of hardness or elasticity, which the superficial layers undergo; for there is no doubt that the operations, by means of which the plate is rendered rough or polished, not producing in the same time the forced displacement of the molecules, which displacements sometimes approach and sometimes withdraw in a firm manner the integrant parts, and render the metal more or less hard or elastic, according to the anterior consistence and the mode adopted to give to the surface a greater or less decided degree of roughness or polish.

As to the direction of the action, it is evident, after what has just been said, that the absorbing power diminishes in proportion as the hardness or elasticity of the plate augments. In fact, the bruised tin, cold-beaten by the percussion of the mallet, possesses a more feeble absorbing power than when found in its natural state. This inferiority does not proceed from a more perfect polishing, for we can easily give to a beaten disc a very inferior polish to the other, without rendering its absorption superior to that of a disc not beaten: it is then, in reality, the greatest hardness which causes the diminution of the absorbing power in the plate beaten with the mallet or hammer. Polished copper proceeding from the flattener, and thereby possessing a real cold-beating, augments the absorbing power when it has been scratched, because the furrows expose the softer parts of the interior, and permit the remaining cold-beaten superficial laminæ, of which all the elements were before found constrained by their mutual compression, to relax and dilate themselves in the dissolutions of cohesion opened at the surface. The plate of silver or of gold, melted and slowly solidified, having received a soft polish, diminishes, on the contrary, its absorbing power, when it is scratched, because the point of the diamond compresses a part of the tender surface of the metal, and communicates to it a great degree of hardness.

The influence which the state of hardness or elasticity of metallic plates exercises on the calorific absorption, appears in an evident manner in the following fact, which was communicated to me by M. Saigey, and confirmed by M. Obellaine, Professor of Natural Philosophy at the Polytechnic School, and at the Faculty of Sciences of Paris:—Dulong had caused to be constructed two large conjugated mirrors, of cast metal, perfectly smooth, and polished all round. On putting this piece of apparatus into experiment, he was quite asto-

ne necessary to take particular care of depolishing gold or silver by means of the file, or of emery, which, notwithstanding repeated washings, will always leave traces, more or less abundant, of heterogeneous matters, encrusted in the metal, and will produce on the inoxydable surface the same effect which produces oxydation on a plate of copper or any other metal, alterable by the action of the air.

nished to find it less active than another couple of mirrors of beaten metal, of much smaller dimensions, which had been for a long time among the instruments of the Faculty. It was not then known to what cause to attribute this singular anomaly; it was supposed to proceed only from a difference in the quality of copper employed in the construction of the two instruments. It is now, however, well known to be an immediate consequence from the principle we have just made known. The stiffer mirrors were necessarily less beaten, and, consequently, less hard and elastic than the mirrors finished with the hammer; they ought, therefore, to absorb a greater quantity of heat, and to give a less reflection. Thus, to have good calorific reflectors, it will not suffice to polish their surface; but it becomes necessary that the metallic plate of which they are composed be well beaten, in such a manner as at the same time to communicate to the surface of the metal a regular surface, the most beautiful polish possible, and a high degree of hardness and elasticity.\* This consequence, which may be deduced by analogy from our first experiments on the emissive power of polished and radiated surfaces, had not escaped the perspicuity of M. Saigey, who since then has made a very happy application of it in the construction of conjugated mirrors and other apparatus destined for the reflection of heat.

The new theory, which takes away from points their pretended influence in the calorific absorption, and attributes it to changes of hardness or elasticity, receives, besides, a striking confirmation by the constancy of the absorbing power which we remark in all bodies that cannot preserve the state of compression which we impress, by any mechanical means whatever, on their superficial laminæ. We see, in fact, that a disc of marble, of jet, or of ivory, absorbs as much heat in its natural state as after having been brought to the highest degree of polish, or scratched with coarse grains of sand or emery. It is in these sorts of substances that the procedures exist, which develop the asperities, or which make them disappear, without any permanent alteration, as in the metals, of the hardness or elasticity of the superficial laminæ. I will add, in fine, that in the course of my experiments I have never been able to recognize any variation in the heating of bodies exposed to calorific radiations, when they are successively painted with the same colouring matter, ground to different degrees of fineness. Here, as in the case of discs of marble, of jet, or of ivory, there is a deviation, greater or less, in the regular disposition of superficial points, without any appreciable change of hardness or elasticity.

\* The great influence which the elasticity or the hardness of the superficial laminæ seems to exercise on the calorific reflection of metals—an influence much more decided than in the analogous case of light—holds without doubt very near to the nature of heat. It seems desirable that it should be made the object of a profound examination on the part of geometers, who now study, under all aspects, the vibratory movements of the fluid, whence we suppose to derive all the phenomena of light, of heat, and of the chemical action which the rays of incandescent bodies possess.

When shewing the insufficiency of the admitted theory on the action of asperities in the radiation of bodies, it has been objected that the irregularities of the surface ought necessarily to vary, in virtue of the reflection, the quantity of heat which passes by a given point. The same objection may be raised in reference to absorption. We will observe, at first, that, in speaking of asperities of the absorbing or radiating surface, we understand solely the small irregularities produced by the depolish; for it is evident that very sensible protuberances, or decisive hollows, may act as real reflectors, and accumulate a greater quantity of heat in certain directions. We shall afterwards remark, that this is not the action of a general law, but of a particular fact. In scratching certain polished metallic surfaces, we obtain an augmentation in their emissive and absorbing power. This augmentation cannot be attributed to the reflection of points, or to any other *immediate action* of the asperities, since we have seen,—1st, that the rays do not exert any influence on non-metallic surfaces; 2nd, that the effect changes considerably with the nature and the state of the plate employed; 3rd, that the metals, unalterable by the air, being conveniently prepared, give an inverse effect, and that then the presence of asperities diminishes the emissive and absorbing powers in place of augmenting them: this latter argument appears to us to be decisive. Thus, the augmentation of the radiating and absorbing force, which had long been remarked of metallic plates, presents only a special case. The indifference and the diminution which we obtained, at a later period, in marble and silver suitably prepared, are also particular cases, in so much that the variations introduced by the polish and the depolish, in the absorbing and emissive power of substances susceptible of becoming bright and smooth, have not a character of generality, and change to the contrary with the nature of bodies and the state of molecular equilibrium impressed on these superficial layers. However, these alterations are observed solely in metals, and we know that these metals undergo, under the action of mechanical forces, permanent modifications in their specific gravity, the hardness and elasticity of their superficial coats. Now, these modifications are the sole effects known. In attributing to them the observed changes in the radiation and absorption, we do not, in reality, make any hypothesis; we solely announce the phenomena under a new form, expressing some previously unknown conditions, with the experiments which we have described in this note.

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## PHOTOGRAPHY.

M. Arago laid before the Academy an admirable plate that M. Fizeau obtained by depositing copper on a Daguerriene proof, fortified by salts of gold, by the aid of the *galvanoplastik* method.

This plate of copper is distinguished from anything which had been expected of this kind, by its astonishing perfection, and also by the circumstance, not less remarkable, that in the operation the Daguerrienne proof did not undergo any alteration.

M. Arago presented, at the same time, two portraits, taken from nature, by the photographic process, but by the aid of a darkened chamber, in which the single lens is replaced by four lenses. Assisted by this substitution, and some other modifications in the chemical part of the operation, *M. Geoffrey de Dreger*, of Vienna, obtained a portrait without the necessity of exposing the person whose portrait he produced to the rays of the sun.

[A well-wisher to truth and just scientific claims has directed our attention to the two following papers, which shew that its author observed perfect sheets of precipitated copper in his battery even earlier than December, 1836, which is previous to anything published on the subject of electrotyping by any other person. We have some reason to think, also, that the battery here described is about the first, on a large scale at least, that was ever excited by a solution of sulphate of copper. But we shall return again to both these subjects; hoping, also, in the meantime, that our readers will favour us with such facts as they may be in possession of, to enable us to render each claimant his due.—EDIT.]

*On Voltaic Electricity, and on the Effects of a Battery charged with Sulphate of Copper.* By Mr. W. DE LA RUE.

The greatest effect being always produced in those voltaic arrangements where the chemical agent exerted an action on only one of the metals constituting the battery, it occurred to me to use a saturated and perfectly neutral solution of the electro-negative metal, provided the other was capable of effecting its decomposition. I therefore tried the effect of a saturated solution of sulphate of copper\* in an elementary voltaic battery of the ordinary construction. The zinc plate was four inches by two, the copper completely surround-

\* Daniell uses sulphate of copper, but not as the exciting agent.

[Professor Daniell's object was to obtain a voltaic combination constant in its effects while the connection is completed, and totally inactive when the circuit is interrupted. Sulphate of copper, used as an exciting agent, he found unavailing for this purpose, and therefore relinquished this employment of it in his battery. That it did not escape Prof. D.'s attention, the following passage from his paper on Voltaic Combinations, in the first part of the *Phil. Trans.* for 1836, page 117, will show: "Upon adding sulphate of copper, in any considerable quantity, to the liquid in the cells, notwithstanding the amalgamation of the zinc, there was local action enough upon that metal to disengage hydrogen, which, in however small a quantity, was sufficient to commence the precipitation of the copper upon it. Single circles were thus immediately formed by the two metals, and local action increased to such a degree as speedily to cover the zinc with reduced copper." —EDIT. *PHIL. MAG.*]



ing it: with this I was enabled to produce ignition of half an inch of platina wire one-thirtieth of an inch in diameter, and continue it as long as the zinc plate lasted, which, being very thin, was dissolved in a couple of hours. The effects of this battery were considerably greater than those of one made of platina and zinc of the same dimensions, this being immersed in diluted nitric acid.

I afterwards constructed a battery with three four-inch zinc plates connected together; these are immersed in a copper trough with two partitions, so that the zinc should be opposed on both its surfaces to a plate of copper: with this battery, one inch of fine iron wire was kept ignited for four hours. The zinc plate is always partially covered with a coating of copper, *which, however, is NOT DETRIMENTAL to the power of the battery*: the copper plate is also covered with a coating of metallic copper, which is continually being deposited; and so perfect is the sheet of copper thus formed, that, on being stripped off, it has the polish, and even a counterpart of every scratch of the plate on which it is deposited. Besides this, the voltaic influence decomposes the water; the oxygen, uniting with a portion of the copper and hydrogen, being set at liberty. This may be readily shewn by soldering at one end a piece of copper and a piece of zinc, coiling the two to form a small calorimeter, which is to be put into a glass jar filled with a solution of sulphate of copper, and inverted in a vessel of the same; metallic copper and its oxyde will precipitate, and hydrogen gas fill the jar.

Seeing the effects so continuous in a simple battery, I tried a Cruickshank's, of one hundred pairs, each plate exposing to the action of the fluid a surface of twenty-five square inches. This was charged with a saturated *cold* solution of sulphate of copper, to each three gallons of which I added two ounce measures of nitric acid, for the purpose of cleaning the plates and freeing them from oxyde; for half an hour the action was so feeble that I was on the point of emptying the trough, but I soon after noticed that the effect was rapidly increasing; I was then induced to proceed. The batteries attained their maximum of power in three quarters of an hour after charging.

Charcoal points were vividly and continuously ignited, the arc passing through a space of three-eighths of an inch; this experiment was beautifully varied by dipping the charcoal in nitrate of strontian, the arc then being of a crimson colour.

Steel points of wire, a quarter of an inch thick, were then tried; the arc passed through an equal space; the steel rapidly fused, was deflagrated, and by the scintillations produced a beautiful effect.

Copper points, treated in a like manner, produced a green arc, and were rapidly destroyed.

Brass produced a blueish white arc; and the more fusible metals, such as bismuth and tin, produced likewise an arc, but the metal was soon carried from one point to the other, and established a perfect contact.

A piece of platina wire, one-eighth of an inch thick, was rapidly fused, by keeping it at a short distance from a disc of copper, so as to allow the arc to pass from it to the disc.

A *heap* of metallic leaves was burned with rapidity.

*Thick* tin-foil was deflagrated.

Very thick zinc-foil was rapidly consumed. A bunch of needles burned rapidly in mercury; the end of a file was deflagrated in the same manner.

Extraordinary as was the power of deflagrating metals, the effect of igniting was comparatively small; not more than an inch of iron wire could be ignited, though, if only twelve pairs of Wollaston's four-inch plates were used, charged with the same solution, two and a half inches could be kept ignited for some time.

The battery was then tried in decomposing common caustic potash, which it did with facility; the combustion of the potassium evolved, vividly igniting the thick platinum wire used for the negative pole. These experiments occupied about two hours. The charcoal points were then again tried; and if there were any alteration, the power of the battery had increased. Batteries charged in this manner will continue in unabated action for upwards of three hours; in fact, until there no longer remains any copper in the solution. It is worthy of notice, that after the batteries have been in action some time, a large portion of the sulphate of copper is expended, and replaced by sulphate of zinc, *yet the action continues the same*. This naturally suggests using a saturated solution of any neutral salt, common salt, for example, and adding merely as much of the solution of copper as will serve for the time required. It is not unlikely that the effect would be more continuous than with a solution of copper only. I intend trying this, as I am still pursuing my inquiries on this subject, the object of which is to simplify as much as possible the voltaic battery.

At the Marylebone Institution, on Monday, September 12, when a lecture was delivered on this subject by Mr. Hemming, the president, the power used was the hundred pairs of Cruickshank's arrangement before alluded to, and one hundred and thirty-two pairs of Wollaston's four-inch plates, making in all two hundred and thirty-two pairs.

The batteries I charged *before* the commencement of the lecture, and they were not used till an hour afterwards; the effects were very striking. The arc from the charcoal points passed through a space of three quarters of an inch, and the effect continued unabated for as long a time as could be spared for this experiment; soda was rapidly decomposed, and the sodium brilliantly deflagrated: all the other experiments before cited were repeated on a much grander scale. The lecture being concluded two hours and a quarter after charging the batteries, the charcoal points were again ignited to light up the spacious theatre, the gas having been

extinguished. The shock was very powerful, even when taken with the hands dry.\*

Fifty pairs of four-inch plates on Cruickshank's plan suffice for all the above experiments, except the decomposition of the fixed alkalis.

Water was decomposed with extraordinary rapidity by a battery of this description, and also muriatic acid, the chlorine of which bleached a solution of sulphate of indigo in a few seconds.

Its effects on the animal system, as exhibited by Mr. Hemming to the audience, were almost terrific. A rabbit recently killed, an eel, and frogs, were thrown into more violent muscular action than I had ever previously witnessed.†

The *tension* of electricity seems to be greatly increased by this mode of charging the voltaic battery.

Bunhill Row, Sept. 15, 1836.

*On the Effects of a Voltaic Battery charged with Solution of Sulphate of Copper.* By Mr. WARREN DE LA RUE.

(With a Plate.)

*To the Editors of the Philosophical Magazine and Journal.*

GENTLEMEN,

In answer to your query (vol. ix. p. 484,) as to the relative effects of batteries charged with sulphate of copper or with acids, I beg to submit to your attention the following facts and deductions.

It is well known that in connecting the poles of a battery with a definite length of wire, the wire will become ignited, and continue so for an exceedingly short space of time after immersion in an acid; and if the battery be immersed without connecting the poles, and allowed to remain for a few minutes, and the connection be then made—with the same length of the same wire—no ignition whatever is produced.‡ As in this latter case no zinc can have been

\* [As similar experiments to those here detailed have been performed with batteries of no extraordinary dimensions, charged in the usual way, it would have been more satisfactory had the author informed us of the size and number of the plates requisite to produce the same effects when sulphate of copper was not employed. We refer our readers who are interested in the philosophical investigation of this subject to an admirable Essay by Dr. Marianini, of Venice, of which an abridgment will be found in the *Annales de Chimie et de Physique*, vol. xxxiii. p. 113. In his investigation of the various causes which influence the energy of the pile, he has been led to examine the effect of different liquid solutions, and gives a table of the relative advantages of forty-nine acids and salts, one part of each being dissolved in one hundred of distilled water.—EDIT. PHIL. MAG.]

† [That a battery of two hundred and thirty-two pairs of four and five-inch plates, or even of a hundred pairs, should violently convulse rabbits, eels, and frogs, is by no means an extraordinary result. The really terrific experiments made by Dr. Ure, on the murderer Clydesdale, at Glasgow, were performed with a voltaic battery consisting of 270 pairs of four-inch plates, charged with dilute nitro-sulphuric acid.—EDIT. PHIL. MAG.]

‡ A battery regains its former power by exposing the plates to the action of the atmosphere.

deposited on the copper plate, prior to the connection of the poles, it follows that this decrease of power must result from some other cause. At the moment of immersion in dilute acid, say sulphuric, the electricity is produced by the combination of the acid with that portion of oxyde which is in perfect contact with the zinc plate: when this thin coating of oxyde is removed, the zinc plate is then oxydized at the expense of water, hydrogen being set at liberty; and as the hydrogen assumes the gaseous form, it annuls or carries off a large portion of the electricity.\*

If sulphate of copper be used in charging the battery instead of acid, oxygen is supplied to the zinc by the oxyde of copper; no evolution of gas, therefore, takes place;† and the action is thus rendered continuous, the effect being fully equal to that momentarily produced by immersion in acids. The fusion of metallic points of *very large dimensions*, the decomposition of fixed alkalis, &c. &c., cited in my former communication as the effects of a voltaic battery of 100 pairs on Cruickshank's construction, cannot be produced by the same battery when charged with acid, the momentary power being exhausted before the battery can possibly be brought into action.‡

The following is an experiment to ascertain the effect on the battery produced by the deposition of copper on the zinc plate. Fig. 10, plate ix. represents one of the zinc plates of the battery: round it, at (a), are placed four copper wires one-tenth of an inch in diameter; these are each attached to the plates by a drop of solder. Fig. 11 shows the zinc plate surrounded by the copper as in Wollaston's plan: the battery consists of twelve such series. It is clear that there are four small local currents in each cell; yet the power of the main current is increased, and not diminished, as I conceived it would have been. From the proximity of the copper wires to the zinc plate, there is no deposition of copper on the zinc plate; it adheres to the copper wires so firmly that it is exceedingly difficult to remove.

I find that amalgamating the zinc greatly increases the power of the battery, and prevents the strong adherence of the copper to the zinc plates, which are therefore cleaned with facility.§ I have a battery of 30 pairs of four-inch plates with amalgamated zinc, which

\* A similar effect takes place in the formation of steam, which causes the gold leaves of an electrometer to diverge with *negative* electricity.

Professor Faraday has shown that a voltaic current ceases to affect a magnetic needle if employed in the decomposition of a solution of iodide of potassium; hydrogen is given off in this case. Again, if a battery be employed in decomposing a solution of a metallic salt, an atom of it will be decomposed for the solution of every atom of zinc; but this does not destroy the current, for an atom of water is at the same time resolved into its elements.

† The exceedingly small quantity produced by the local action cannot be taken into account.

‡ This fact I am ready to prove by actual experiment to any scientific gentleman who will do me the favour to call for that purpose.

§ The zinc plates are amalgamated by rubbing them with dilute nitric acid and mercury; the mercury is allowed to be absorbed by the zinc plate, and the operation must then be repeated, which is a requisite condition.

is well adapted to the use of sulphate of copper ; it is more economical in its construction \* than any now in use, and possesses this advantage, that the zinc, when worn out, may be easily replaced. Fig. 6 shows the zinc plate, which must be tinned on the top A † prior to the amalgamation of the rest of the plate ; B B are two slips of wood grooved out to within three-fourths of an inch of the bottom, and intended to retain the zinc plate in its proper position. The copper plates are formed into cells, as represented in fig. 7, five inches square, and one inch wide : E E are two ears of copper, by which the cell is suspended in its place ; A is a slip of copper to form a connection by means of solder to the zinc plate in the adjoining cell. The cells are painted on the outside to protect them from the action of acid. The zinc plates do not descend lower than within three-fourths of an inch of the bottom of the cells, so that the space left may contain the deposit resulting from the decomposition of the sulphate of copper. The cells are supported in a long wooden frame by the ears E E, and retained in their place by tacks driven through them, as represented in fig. 9. Fig. 8 represents a contrivance by which the charge may be renewed while the battery is in action : at the top of each cell may be placed a lip or spout L, a quarter of an inch deep ; these must overhang a wooden gutter running the length of the frame. The solution must be renewed with a funnel having a long neck, the long end being inserted nearly to the bottom of the cell ; when fresh solution is poured in, the spent liquor will run out of the lip into the gutter.

Immediately after a series of experiments, the battery must be emptied, and the plates well cleaned by dashing water between the cells. If this be not immediately attended to, it will be exceedingly difficult to remove the deposit from the cells. A Cruickshank's battery is best cleaned by laying it on its side.

I remain, gentlemen, yours, &c.,

WARREN DE LA RUE.

110, Bunhill Row, Dec. 7, 1836.

P.S. The fact related in your first note (vol. ix. p. 484) shows that Professor Daniell had no intention of employing sulphate of copper in an ordinary battery ; he immersed a zinc plate in sulphate of copper, and found that there was local action, from the deposition of copper ; but he went no further.

\* The one I have, cost altogether £3 ; but as I put this together myself, a similar battery constructed by workmen would, of course, be rather more expensive.

† This is effected by filing the top smooth, wiping it over with a little muriate of ammonia, and then dipping the top in a ladle of melted tin, a little tallow being placed on the tin to prevent the surface from oxydating.

REMARKS.—We have not only given Mr. De la Rue's valuable papers entire, but also the whole of the notes of one of the Editors of the *Philosophical Magazine*, in which work these papers first appeared, which notes are, no doubt, suitable appendages: notwithstanding, however, we are very far from thinking that they are any ways detrimental to the true merits of Mr. De la Rue's labours, nor can they in any way affect them. A description of Mr. Young's Voltaic Batteries will be found in another part of this number.—EDIT.

*On Indigo.* By J. C. BOOTH.\*

PART I.

*The Constituents of Ordinary Indigo.*—Of all colouring materials employed in dyeing and printing on vegetable or animal fibre, indigo claims pre-eminent rank, whether in regard to its beauty, durability, or the variety of methods of application, and indeed, in respect to its fastness or permanency, it is unequalled by many, and scarcely excelled by any of the colours derived from the mineral kingdom, when exposed to ordinary atmospheric agents. These considerations, and the fact of its very extended employment in colouring processes, point it out as worthy of peculiar attention and close investigation, in order to a thorough knowledge of its characteristic properties and chemical relations, which, although not sufficiently examined, have nevertheless been rendered more intelligible, chiefly by the experiments of Berzelius.

In the state in which it occurs in commerce, it has long been known to be composed of a blue colouring matter, with various other ingredients, which were very imperfectly understood, until Berzelius proved that it contained four principal ingredients, together with small quantities of others. These are indigo-gluten, indigo-brown, indigo-red, and indigo-blue.

1. Indigo-gluten is obtained from the indigo of commerce, by treating it in fine powder with dilute sulphuric acid, which extracts it, together with salts of lime and magnesia. The insoluble remainder is boiled several times with water, which takes up more gluten than the acid water. The solution is saturated by carbonate of lime (chalk or marble), filtered, evaporated to dryness, and treated with alcohol. By evaporating the tincture to dryness, the gluten remains as a yellowish, or yellowish brown, transparent and shining

\* From Journal of the Franklin Institute.

varnish. In its behaviour towards reagents it resembles gluten, differing from it by its solubility in water, and its want of adhesiveness; it differs from vegetable albumen by its solubility in alcohol, and its not coagulating by ebullition.

2. Indigo-brown is obtained by treating the insoluble remainder, after extracting the gluten, by a concentrated solution of caustic potassa, with the assistance of warmth. The mass becomes instantly black, and swells up to a porous paste, in proportion as the brown colour dissolves. It is diluted and filtered, and the filter slightly washed. The solution neutralized, and then acidulated by sulphuric acid, suffers the brown to precipitate, which may then be washed. As it still contains a portion of indigo-blue, it is dissolved in carbonate of ammonia, evaporated to dryness, dissolved in a little water, and filtered, by which operation the blue, with a portion of the brown, remains on the filter. It is exceedingly difficult to obtain it perfectly free from other substances, so that it cannot be said that we understand the nature of the pure brown substance. In its purest form it is a transparent, shining, brown varnish, slightly soluble in water, with neither an alkaline nor acid reaction. It combines eagerly with acids, forming combinations, which are difficultly soluble in water. It unites so powerfully with alkalis, that the resulting compounds, although soluble in water, give no alkaline reaction with reddened litmus paper.

3. Indigo-red is obtained by boiling the remainder from the preceding operations, with alcohol of 0.83. Being difficultly soluble in alcohol, it is requisite to submit the residue to repeated boilings. After some time, the red solution passing through the filter, assumes a blue colour, from the presence of indigo-blue. When the alcoholic liquid is concentrated by distillation, a blackish brown powder deposits, which, separated by filtration and washing, is indigo-red. By solution in alcohol or ether, and spontaneous evaporation, it remains as a dark red powder. It is insoluble in water, alkali, or dilute acid. Concentrated sulphuric acid dissolves it with a dark yellow colour, and if wood be placed in the diluted solution for several hours, it discolours it, while the wood receives a colour varying from a yellowish brown to red. Chlorine water renders indigo-red yellow and soft, like wax; but after exposure to air, it assumes its original character.

4. After the three preceding substances are separated, the indigo-blue which remains is not absolutely pure, but may be rendered so by the following treatment. It is mingled with quick lime (about twice the weight of the crude indigo), which is slacked to a powder immediately before its employment, put into a flask capable of containing one hundred and fifty times as much water as indigo, the flask is nearly filled with boiling water, and shaken. Sulphate of iron (copperas) is added (about two-thirds the weight of the lime) in fine powder, or dissolved in a little boiling water, the flask corked tightly, and well shaken. It is suffered to stand a few hours in a

warm place, when the liquid assumes a lemon or dark yellow colour, in proportion to its concentration. As soon as the liquid has settled, the clearer portions are drawn off, and the remainder filtered through paper. The indigo-blue, separating from the liquid when in contact with the atmosphere, carries down with it a portion of the foreign substances, an inconvenience easily obviated, by pouring the yellow liquid into dilute muriatic acid, which retains them in solution. The precipitate is well shaken until it assumes a full blue colour, then thrown on a filter and thoroughly washed. After it becomes dry, it is no longer of a blue colour, but has a shade of purple, and by friction exhibits a metallic lustre resembling that of copper. Hence the strength of the purple indicates, in some measure, the amount of blue in raw indigo.

Indigo-blue, in its pure form, has neither taste nor smell, exhibits neither the reaction of an acid nor a base, and, in short, is one of the most indifferent substances in its chemical relations. It burns, with difficulty, in the open air, with much smoke, and leaves a charcoal which burns with difficulty. If heated in a close vessel, without the access of air, it is converted into a purple vapour, which is gasiform indigo-blue, and condenses on the cooler portions of the vessel in crystalline scales, of a purple colour and metallic lustre. A considerable quantity of the blue is decomposed by the operation. The same crystals may be obtained from commercial indigo, but they are rendered impure by the sublimation of indigo-red. To perform the operation on a small scale, it is only necessary to place a cone of strong brown paper over a slightly concave tin dish of two or three inches diameter, and heat the latter over a lamp until the paper begins to brown, which heat is maintained until the greater part of the indigo is sublimed. The interior of the cone will be thickly coated with crystals, which can be purified, if requisite, by repeated boiling in fine powder with alcohol, which removes the red.

Indigo-blue is insoluble in water, alcohol, ether, olive oil, or spirits of turpentine. It is not affected by dilute acids, nor caustic alkalis. Chlorine instantly decomposes it, and renders it yellow. It is readily de-oxydized by substances which have a strong affinity for oxygen, provided an alkali or alkaline earth be present, in which case the reduced indigo combines with the strong base. It is dissolved by concentrated sulphuric acid, but changed in a very remarkable manner. It is farther decomposed by nitric acid, giving rise to new and singular products. These chemical relations of indigo will occupy our attention in the succeeding part of the essay.

It appears, therefore, from the preceding, that there are four principal organic constituents in indigo, besides others in smaller quantity. Of these, the blue rarely attains 50 per cent. of the weight of the raw material; the red and brown are each present in larger proportion than the gluten. Beside these, there are mineral substances contained in it, either accidental or adulterations, such as silica, lime, magnesia, oxide of iron, alumina, potassa, and a little phosphoric acid.



## PART II.

*The Chemical Relations of Indigo-blue.*—The blue substance contained in the indigo of commerce, being the only one of importance in colouring operations, its singular and unique action with many of the chemical agents next demands our attention. According to the method of preparing the pure blue colour, described in the preceding part of the essay, it will be observed that a protosalt of iron was precipitated by lime, the protoxyde becoming peroxyde, while the blue colour of the indigo disappeared, as it united with the lime. Here, then, according to the most commonly received theory, the indigo-blue has been de-oxydized, and combines with the lime, after the nature of an acid. This important change first requires attention.

1. *Reduction of Indigo-blue.*—The de-oxydation may be effected by any substances which have a strong affinity for oxygen, such as phosphorus, the sulphurets of potassium, calcium, antimony, and many sulpho-salts, particularly the sulph-arsenites, by protosalts of tin, iron and manganese, farther by filings of zinc, iron, tin, &c. Even organic materials, in the process of fermentation, have a similar action. The presence of a free alkali or alkaline earth is essential to the change, and seems to act by its presence or catalytic influence, as metallic zinc decomposes water by the presence of sulphuric acid, which unites with the newly-formed oxyde; for none of the above named substances will de-oxydize indigo unless an alkaline base be present to combine with it when reduced. Thus, in the process described in Part I, the reduced indigo combines with the lime forming a soluble compound, while the iron is peroxydized at the expense of the oxygen of the indigo-blue and precipitates. After the solution of the calcareous compound has become perfectly clear, it is drawn off by a syphon into another flask, so as to fill it even to overflowing, by which a small portion of the blue, which has re-formed, may be carried off. A few drops of concentrated sulphuric or acetic acid, previously deprived of air by boiling or in vacuo, are now added, and the flask closed by a well ground stopper, whereupon an abundant white and flocculent precipitate appears, which slowly collects at the bottom of the vessel. This is reduced or de-oxydized indigo. The clear liquor is drawn off, and the precipitate, placed on a filter, is washed with well boiled water, (during which operation it gradually becomes greyish green,) then dried between paper and in vacuo, or even in the air at a very moderate temperature, 75° Fahrenheit. The greenish shade it assumes appears to be an intermediate state of oxydation between the white and blue. Reduced indigo is white, somewhat crystalline; has neither taste nor smell, does not redden litmus paper, and is insoluble in water. It dissolves in alcohol and ether, with a yellowish colour, from which it separates as indigo-blue, either by exposure to air or boiling down. In a moistened state, it becomes deep

purple in a few hours; in dry air, it changes to blue, only after several days. So strong is its attraction for oxygen, that it cannot be kept even in closely stoppered bottles. By heating the dry mass cautiously in the open air, when the temperature rises to a certain point, it suddenly becomes dark and purple coloured; a phenomenon resembling the sudden oxydation of a metal. By a higher heat it rises as a purple gas. It appears to have no affinity towards dilute acids, but dissolves instantly in concentrated sulphuric acid, with a deep purple colour. The nature of this solution is unknown; but may it not arise from a reduction of a portion of sulphuric to hyposulphuric acid, which combines with the newly formed indigo-blue? It combines forcibly with strong bases, being dissolved by carbonated and caustic alkalis, and by caustic baryta, strontia, and lime, with a yellow colour.

The combinations of reduced indigo with the bases cannot be obtained pure in a dry state, for even under the air-pump they change so far as to conceal their characters. Lime forms two combinations; the one exactly saturated with the indigo is soluble in water, the other with an excess of lime, insoluble, and of a citron yellow colour. Magnesia likewise forms a compound somewhat less soluble. Other compounds may be obtained by introducing a crystalized salt into a saturated solution of reduced indigo. Salts of alumina, protoxides of iron and tin, and oxide of lead, precipitate white compounds, which become instantly blue in the air; oxyde of cobalt and protoxyde of manganese, form green, and oxyde of silver, brown and black precipitates.

It appears, then, that reduced indigo, although presenting no acid character in itself, combines with bases after the manner of an acid, forming both soluble and insoluble compounds; that its attraction for oxygen is powerful, becoming converted thereby into indigo-blue, which is an unusually indifferent substance.

When indigo is obtained from plants, it does not exist in the juice of the plant as such, but is formed after its expression, and by contact of the air. It is, therefore, highly probable that it is contained in it as reduced indigo; but as the latter is insoluble in acids, and requires the presence of a base, while the infusion of indigo plants always reddens litmus, it remains to be determined, by future experiments and observations, in what state it exists in the juice of the plants.

2. *Soluble Indigo-blue*.—Sulphuric acid has a peculiar decomposing action on various organic substances, combining with a portion of the generated products, and although retaining its acid character, ceases to exhibit the properties of sulphuric acid; for the body combined with the acid cannot be separated from it by bases, but enters into the composition of its salts, which are very different from sulphates. Indigo-blue is one of these substances, and exhibits properties, the theoretic nature of which remains a mystery.

When indigo-blue, purified from the brown, red, &c., as described

in Part I, is treated with fuming sulphuric acid, it combines with it rapidly, evolving heat, and no sulphurous acid; even dry sulphuric acid produces the same result. Dilute acid has no action, and the stronger the acid, the more colouring material is dissolved. Thus while six parts of the fuming acid are requisite for the solution, twelve parts of strong oil of vitriol are required, and then usually with the assistance of a temperature not higher than  $212^{\circ}$  Fahrenheit. Such a solution communicates a distinct blue colour to 500,000 times as much water. As the exact nature of the combinations is not understood, we shall follow the ordinary nomenclature, and call the two acid compounds resulting from the solution, ceruleo-sulphuric and ceruleo-hyposulphuric acids; a third compound also results, called indigo-purple, or plenicine, but as it is unimportant, the mention of it may be sufficient. The more fuming the acid, the more blue hyposulphuric is formed, and the less residue of indigo-purple, while strong oil of vitriol produces more of the blue sulphuric, and frequently leaves a large portion of purple.

These acids may be prepared by digesting the pure indigo-blue in a dry state, in twelve parts of very concentrated sulphuric acid, adding the powdered colour little by little to the acid, and suffering the mixture to stand twenty-four or forty-eight hours, according to the temperature, which should not be above  $211^{\circ}$ ; it is then diluted with thirty to fifty times its bulk of water, and filtered. What remains on the filter is indigo-purple. Wool or flannel, thoroughly cleansed by soap, a very dilute solution of carbonate of soda (1 to 100) and water, is immersed and digested in the liquid at a moderate warmth, by which the blue acids are extracted, and colour the wool of a deep blue. When all the colour is thus taken up, the wool is thoroughly washed with water, pressed, and put into a very dilute solution of carbonate of ammonia, whereupon the acids leave the wool and combine with the ammonia. This solution is evaporated to dryness at  $140^{\circ}$ , and treated with alcohol of 0.833, which dissolves the ceruleo-hyposulphate of ammonia, and leaves the blue sulphate undissolved.

Ceruleo-sulphuric acid is obtained by dissolving the last-named salt in water, and precipitating with acetate of lead, which throws down ceruleo-sulphate of lead. By stirring this salt with water, and passing sulphuretted hydrogen through it, sulphuret of lead is precipitated while the reduced blue remains in the yellowish liquid. It is de-oxydized blue in combination with sulphuric acid, which, by exposure to the air, becomes the ceruleo-sulphuric acid. The blue hyposulphuric acid is obtained by precipitating the above alcoholic solution of the ammoniacal salt by a solution of acetate of lead in alcohol, and then proceeding as for the sulphuric acid. In either, the sulphuretted hydrogen should be removed, and the liquid should not be filtered until it is perfectly blue.

The blue acids have a powerful affinity for bases, and the resulting compounds are not to be regarded as double salts, for in the

ceruleo-sulphate of baryta the sulphuric acid exactly saturates the baryta, so that the blue colour appears to combine with the salt, somewhat like water of crystallization in ordinary salts. It will appear from this that it still remains for the chemist to determine in what manner the blue colour combines with the acids. These ceruleo-salts have all a fine blue colour, whether soluble or otherwise, taste slightly saline, and very strongly like indigo, and require a considerable amount of heat to decompose their organic colour. The sulphates of the alkalis are scarcely soluble in alcohol of 0.84, and are precipitable by colourless alkaline sulphates, and even by other salts. The corresponding hyposulphates are soluble in alcohol, and scarcely precipitated by other salts. Both series of salts, when soluble in water, leave, by evaporation to dryness, an uncrystallized mass, with a cupreous metallic lustre, surpassing that of insoluble indigo-blue. The ceruleo-salts of potassa, soda, and ammonia, may be prepared pure by digesting the blue wool, alluded to above, in the carbonates of those alkalis to saturation, evaporating to dryness, and treating with alcohol, which dissolves the blue hyposulphates, and leaves the ceruleo-sulphates. The corresponding compounds of lime and magnesia are prepared, as those of the alkalis may also be, by treating the solution of indigo-blue in sulphuric acid by their carbonates, and by farther separating the two series from each other by alcohol. The salts of baryta and lead may be precipitated from the soluble alkaline compounds by solutions of those metals.

The blue colour in the two acids is susceptible of a de-oxydation similar to insoluble indigo-blue. If filings of zinc or iron be placed in their solution, the metals are oxydized at the expense of the blue colour, for hydrogen is not evolved; and with an excess of acid, the solution is colourless or yellowish, but instantly becomes blue by contact with oxygen or atmospheric air. It is the most delicate of all tests for the presence of oxygen gas. The blue colour is more easily reduced in the salts than in the acids, and most readily when there is an excess of base. A beautiful experiment illustrates this property. Protosulphate of iron may be dissolved in a neutral blue solution, and heated in it without inducing reduction; even a large portion of protoxyde of iron may be precipitated with alkali, without producing a change; but the moment we add an excess of alkali, the blue fluid instantly changes to yellow. By saturating the solution with acid, it soon becomes blue, and may again be rendered yellow by a new excess of alkali. It is remarkable that indigo-blue should suffer its peculiar deoxydation, but more so that this soluble blue should resemble it in this feature, although differing in most others.

The blue colour is not so firmly united to the two acids of sulphur as not to form other compounds; thus, by mingling chloride of calcium with a blue solution, and adding phosphate of soda, a fine blue phosphate of lime precipitates; if carbonated alkali be added, a less brilliant ceruleo-carbonate of lime is thrown down. Even

tannate of oxyde of lead may be obtained as a blue precipitate, by throwing acetate of lead and tannin simultaneously into the solution. It would be highly desirable to multiply experiments of such a character, as the transposition of the blue colour may yet prove of service in dyeing and printing operations.

Soluble indigo-blue is as fugitive as many vegetable sap colours, suffering alteration from the solar ray, heat, nitric acid, caustic alkalis, &c., forming new bodies which unite with the sulphuric and hyposulphuric acids, and give rise to new acids. If ceruleo-hypo-sulphate of baryta be evaporated to dryness in a water bath, it becomes green, and contains *virido-sulphuric acid*, which may be obtained like the blue sulphuric. If one part of ceruleo-sulphate of potassa be dissolved in fifty parts of lime-water, and heated in a closed vessel, for several hours, the solution becomes of a purplish red colour; by filtering, and passing carbonic acid through the solution, evaporating to dryness, and treating with alcohol, a small portion is dissolved with a red colour. The insoluble residue dissolves in water with a dark purple-red hue, and being precipitated by neutral acetate of lead, gives a salt containing a new acid, the *purpuro-sulphuric*. If the blue sulphate of potassa be similarly treated, in an open vessel, with lime water, the colour of the solution graduates through green, purple, and red, into yellow; but by ceasing when it is red, precipitating by carbonic acid, and evaporating to dryness, alcohol extracts a yellow compound, while the residue becomes red. Sugar of lead throws down from the tincture a citron yellow salt, containing *flavo-sulphuric acid*, while the remaining fluid is red. The red residue insoluble in alcohol, and the last red solution contains a reddish acid, which, by solution in absolute alcohol, yields *fulvo-sulphuric acid* of a reddish-yellow tint; and the residue insoluble in absolute alcohol, gives a fine red solution with water, containing the *rufo-sulphuric acid*. A cursory view of this subject might lead to the supposition that these singular, varied, and beautiful compounds, although interesting to the chemist, can never be rendered available in practical operations; but we have already too many instances in the arts of substances now employing the hands of numerous artisans, which only sprang into existence under the touch of the chemist, to listen to such assertions. When the existence of chrome was first made known to the world, who could have imagined that it was destined to play an important part in the arts; or even when sand and alkali first fused together, by an accidental intensity of heat, who could have known that the resulting glass would eventually prove a source of many of the comforts and conveniences of mankind? Neither can we now assert that an intimate knowledge of the varied and curious properties of indigo arising from the labours of the chemist, can prove of no benefit to manufactures.

3. *Action of Nitric Acid on Indigo*.—This acid has a peculiar decomposing action on a variety of organic substances, giving rise to

a host of new products, which are not yet well understood. Thus, by its action on indigo, it produces several distinct acids, of which but little is known, not so much from a want of extended observation and experiment, as from the extreme difficulty attending such chemical investigations. If we employ nitric acid, of 1.28 sp. gr., diluted with an equal bulk of water, and add to it one part of finely powdered indigo, as fast as it is decomposed, with the assistance of warmth, two acids form in the solution, and by concentration, precipitate together in a crystalline form. By solution in boiling water, and cooling, fine needles separate, which are *nitroanilic acid*, formerly called indigotic acid. It may be purified and separated from the *nitropicric acid*, which is also formed, by boiling its solution with freshly precipitated carbonate of lead, when the nitroanilate of lead remains in solution. When purified, the acid crystalizes in fine white needles, of a strong acid taste. It combines with the alkalis, earths, and many metallic oxydes, forming yellowish or reddish compounds.

The nitropicric acid may be formed more readily by digesting one part of indigo with eight to ten parts of moderately strong nitric acid, with the assistance of a gentle heat, and as soon as the liquid ceases to evince decomposition, and is still, by heating it to boiling, and adding, from time to time, nitric acid, as long as nitric oxyde is evolved. The fluid, suffered to cool, deposits crystals of the acid, frequently called the carbazotic, and formerly Welter's Bitter. It combines with bases, usually forming yellow salts. The salt with potassa being rather insoluble in water, and wholly so in alcohol, this acid has been employed to detect the presence of potassa. It has also been recommended by Bracconot as a remedy for intermittent fevers. Excepting these, the salts of these two acids have received no practical application. In regard to the manner in which their elements are combined, it is difficult to hazard a conjecture; but Berzelius rather regards them as organic bases combined with nitric acid. Thus the nitroanilic acid may be represented as  $(C^{14} H^8 O^4) + NO^5$ ; and the nitropicric as composed of  $(C^{12} H^4 N^2 O^3) NO^5 + (HO) NO^5$ ; and when the latter combines with a base, as potassa, the HO, or atom of water, is replaced by that base; thus  $(C^{12} H^4 N^2 O^3) NO^5 + (KO) NO^5$ . It is probable that much time will not elapse ere these dark regions in organic chemistry will be illuminated by advancing science. They have been presented, as well to give a complete and yet condensed view of the subject of this essay, as to point out the numberless modifications of which a single substance is susceptible. In the succeeding part of the essay, we shall devote a few pages to the practical operations connected with indigo.

## PART III.

The indifferent character of indigo rendering it almost impossible to combine it with organic fibres, it is usual to submit it to one of two operations in order to effect its combination, either by reducing it to colourless indigo, or by solution in sulphuric acid, both of which processes were described in Part II. The modes of reduction vary according to the nature of the reducing material, for in the process referred to, it takes place in the cold, but where fermentation is resorted to, heat is usually applied.

1. *Copperas, or common blue vat, cold vat.*—The principles of this process, as well as the mode of conducting it, were described in parts I and II, but the proportion of materials may vary according to their several qualities. Thus the following recipes are given among others:—

Indigo,	1	1	1	6
Copperas,	2	3	4	15
Lime,	3	4	2	20
Potash,			2	4

The copperas should be as free as possible from peroxyde of iron, which exerts no reducing influence, and from sulphate of copper, which would reoxydize the reduced blue colour. The lime is supposed to be in the dry hydrated state, that is, slacked with a quantity of water just sufficient to reduce it to a fine powder, and a due proportion of this hydrate should be employed, for an excess forms an insoluble compound with the reduced indigo, occasioning an equal loss in the vat. This vat is adapted to silk, cotton, and linen, which are dipped into the yellow liquid after it has settled, suffered to remain in it a short time, taken out and exposed to the air, the oxygen of which acting on the reduced indigo, converts it into its characteristic blue shade. A weak bath and a single dipping may be sufficient for a light blue, but any desired shade may be attained by a more concentrated bath and more frequent dipping, observing to expose the fabric to the air after each dipping, until it receives its full depth of blue. After the last operation, the dyed materials are dried, treated with very dilute sulphuric or muriatic acid, to separate the lime, and finally rinsed in pure water. It appears, then, that the chief object to be attained by the reduction of indigo, is to render it soluble, so that it may enter into a fibrous texture, and then and there be converted into the blue by the operation of the air.

2. *Orpiment and tin vats.*—These are chiefly employed in printing goods, but their use depends on the same principles as those of the cold vat. For the former, one part finely powdered indigo, two parts potash, and 175 parts of water, are boiled; one part of freshly slacked lime is added, and the whole again boiled; and lastly, one part of orpiment (sulphuret of arsenic) is added, and the mixture suffered to stand. It is usually thickened with gum, and applied by the hand or block. In this operation the arsenic and sulphur

are oxydized at the expense of the indigo, which, in its reduced state, forms a soluble compound with the lime, while the generated acids of sulphur and arsenic combine with the potassa. The operation being tedious, and requiring much care in preparation, has given place to the frequent employment of oxyde of tin instead of orpiment.

The following compositions are employed, the first for the block, and the second for cylinder printing :—

Caustic soda lye,	= 3½ gals.	3½ gals.
Hydrated protoxyde of tin,	= 5¼ lbs.	5 lbs.
Finely ground indigo	= 3½ lbs.	3½ lbs.
Raw sugar,	= 21 lbs.	
Venice turpentine,		3 lbs.
Gum,		11 lbs.

The caustic lye should be of speci. grav. 1.15; the protoxyde of tin is precipitated from a solution of protomuriate of tin by carbonate of potassa; the sugar and gum are used merely for thickening. The operation with the protoxyde of tin depends on its affinity for oxygen, which converts it into the peroxyde, while the indigo is simultaneously reduced and combines with soda. Turpentine is employed, in the second instance, in order to obviate the rapid reoxydation of indigo in the atmosphere; it being less necessary in the first case, since the mixture is kept more excluded from the atmospheric action. If a solution of muriate of tin be substituted for a portion of the precipitated protoxyde, the mixture is less subject to oxygenation.

3. *Warm and fermented, or pastel, vat.*—Woad, indigo, madder, bran, potash, and lime, are employed in this vat; the proportions of which necessarily vary; but the following may serve to show its usual composition: fifty woad, four indigo, three madder, two potash and caustic lime. The iron, copper, or wooden vat, is filled with water, and heated to 160° Fahrenheit, while the four ingredients are introduced; the temperature is maintained several hours, during which, slacked lime is gradually added, until one and one-third of caustic lime has been expended. The vat is now suffered to cool, during which, lime is again added in small portions. A fermentation ensues; the blue colour passes into green, and when the smell of acetic acid is perceptible, the liquid assumes a yellowish colour, and is ready for dyeing operations.

The theory of the process is analogous to that of the cold vat. The madder, woad, and bran, abounding in starch, sugar, gluten, &c., enter readily into fermentation in warm water, in order to maintain which, they abstract oxygen partly from the air, and in part from the indigo; the latter being thus reduced or de-oxydized, forms a soluble combination with potassa, which is partially rendered caustic by lime. A portion of indigo-brown is dissolved with the blue, but is again precipitated by lime. Among other products of fermentation, carbonic and acetic acids are generated, which are neutralised by lime, and hence the gradual addition of this earth in



proportion as they are produced. Woad simply dried is better than the fermented colouring material, for the latter is apt to become putrified, an accident that sometimes happens, and may be remedied by more indigo and alkaline matter in the original proportion of ingredients. A quantity of lime should be added nearly sufficient to neutralise the generated acids, in order to keep the indigo in solution, and yet preserve a slight acidity in the bath, for if the fluid be alkaline, it combines with the extractive matter, and forms an insoluble compound with indigo-blue, which is thus rendered inert. In a healthy state of the bath, therefore, lime has the property of rendering the potash more caustic and powerful, of precipitating indigo-brown, which would deteriorate the blue colour, of keeping the blue in solution, and of neutralising an excess of acidity.

4. *Urinous vat.*—This method of dyeing with indigo has been superseded in larger establishments by the preceding, and is now only practised on a small scale. Its operation depends on the fermentation of warm urine, by which the indigo is reduced, and combines with ammonia simultaneously generated.

5. *Potash vat.*—In this bath, indigo, madder, bran, and potash are employed, the last being added in several successive portions, while the bath is maintained at 122° Fahrenheit. The theory of the operation is similar to that of the pastel vat, excepting that only a small quantity of lime is added towards the close of the operation, to check fermentation, and precipitate indigo-brown. This vat is said to be superior to the pastel vat, although more expensive in materials, by requiring less time in dyeing, penetrating cloth better, by keeping sound for a longer time, and by requiring less outlay in its preparation.

6. *Sulphuric indigo vat.*—The theory of this solution was discussed in Part II. It is usually termed the Saxon blue dye, having been discovered by a Saxon of the name of Barth. As water prevents, more or less, the proper action of the acid, the fine indigo should be dried, and the sulphuric acid boiled, if it be not perfectly concentrated. One part of indigo is gradually added to six to eight parts of the acid, always taking care to prevent the mixture from becoming warm by keeping the vessel in cold water, where the temperature of the air is too high, for the action of the ingredients generates heat, and might destroy a portion of the blue colour. The mixture is suffered to stand thirty-six to forty-eight hours in a moderately warm place, which effects solution, and prevents the acid from attracting moisture. It may then be diluted with any desired quantity of water, and filtered, or drawn off clear. If goods be dyed in this solution, the red, brown, and glutinous matters of indigo are also attached to the material with the blue, which would therefore be deteriorated in its beauty of shade. To avoid this, the indigo-blue (or ceruleo) sulphuric acid may be prepared by attaching it to wool, as described in Part II, washing with water, digesting

in water containing a little carbonated alkali, and afterwards adding a little dilute sulphuric acid to the solution. By attaching it to the wool, gluten remains; by digestion with alkali, the indigo-red remains on the wool; and by the last addition of acid, indigo-brown is precipitated, while the alkali is supersaturated. The last clear solution communicates a fine blue colour to wool.

Probably the best method of preparing a fine blue is to add to the sulphuric solution of indigo about twenty times its volume of water, and add potash until one-fourth or one-third of the alkali is saturated. The ceruleo-sulphate of potassa precipitates, (see Part II,) which, being dissolved in water and acidulated, produces the finest Saxon blue. The liquid, separated from the precipitate, may be employed for a less perfect shade of colour. Materials to be dyed in the sulphuric blue vat should be dipped into a solution of alum, and then into the blue liquid, to which an excess of carbonate of potassa has been added, by which means a basic ceruleo-sulphate of alumina is attached to the fibres; or they may be dipped into a warm solution of chloride of barium and bitartrate of potassa, and then into an acid solution of the blue liquid, which produces a precipitate of neutral ceruleo-sulphate of baryta; the latter is more permanent than the former, but, at the same time, more expensive.

It is difficult to ascertain the amount of indigo annually employed in dyeing, but it is probable it cannot fall short of 14 to 15,000,000 pounds. In England, about 1,700,000 pounds were imported in 1785, 3,600,000 in 1800, 5,000,000 in 1820, and over 7,000,000 in 1836.

*On the Combinations of the Volatile Chlorides with Ammonia, and on their Composition. (Zusammensetzungsweise.)* By HEINRICH ROSE.

Translated from Poggendorff's *Annalen der Physik und Chemie*. B. lii. s. 1.

(Continued from page 367.)

*The Combination of the Sulphate of Chloride of Sulphur*  
( $\text{SCl}^2 + 5\ddot{\text{S}}$ ) *with Ammonia.*

To become accurately acquainted with the composition of this compound, is in itself of no inconsiderable interest; as by this means, perhaps, some important theoretical inquiries, at this time pursued by chemists, may become determined.

I have, some time ago, prepared this compound, and made known its most important properties.\* It was, in that case, however, not completely pure; for it is exceedingly difficult to completely saturate the sulphate of chloride of sulphur with ammonia. There is no volatile compound of chlorine with which this point is accom-

\* Poggendorff's *Annalen*, bd. xlii. s. 330.

plished with so much difficulty as with this. At the commencement, the ammoniacal gas must be introduced very gradually to the compound, which, at the same time, must be kept very much cooled (*stark erkältet*) in order to avoid all heating; for if the gas be introduced too rapidly, and the vessel not kept sufficiently cool, the mass becomes strongly heated, and assumes a yellow colour. The cause of this yellow colour, which arises from some *sulphat-ammon*, (anhydrous sulphate of ammonia,) I have explained on a former occasion. Its presence is the cause of the solution of the ammoniacal compound giving, with a solution of nitrate of silver, a precipitate of chloride of silver of a yellowish tint, occasioned by a slight intermixture of sulphuret of silver. The compound is far from being saturated with ammonia, even when the glass in which it is prepared and preserved contains free ammonia. After a long time, however, this is absorbed, and new ammoniacal gas must be introduced, and the pieces of the compound broken into the smallest possible pieces. This must be repeated from time to time, and continued until, after long standing, free ammonia is observed in the vessel. It was not till the lapse of half a year that I succeeded in completely saturating sulphate of chloride of sulphur with ammonia.

Carefully prepared, the compound is perfectly white, and completely soluble in water. The best proof of its being saturated with ammonia, is when the aqueous solution does not redden litmus paper in the slightest degree; however, the litmus paper after soaking in the solution, turns red by drying. This is the case with many ammoniacal salts:—with chloride of ammonia,—with sulphate of the oxyde of ammonium,—with sulphat-ammon, and para-sulphat-ammon. With nitrate of silver a perfectly white precipitate should be produced.

The solution behaves itself towards a solution of a salt of barytes, precisely as the solution of sulphat-ammon, as I have already observed. I will here add, that the turbidness of sulphate of barytes becomes denser, especially on the addition of hydrochloric acid. With a solution of chloride of strontium no precipitate is produced, except by boiling, and when, at the same time, free hydrochloric acid has been introduced. By adding a solution of chloride of platinum, only a part of the ammonia is thrown down.

The powdered compound attracts a very small quantity of moisture, as is the case with sulphat-ammon and most pulverized bodies, but does not deliquesce.

1·383 gramme of this compound dissolved in water, was mixed with nitric acid, and then with nitrate of silver. There were obtained 1·2505 gramme of chloride of silver. The oxyde of silver was removed from the filtered solution by the application of hydrochloric acid, upon which an excess of chloride of barium was added. The whole was evaporated to dryness, and the dry mass heated to a slight glowing red heat, and excess of hydrochloric acid and water thrown on it: in short, the treatment was similar to

that of sulphat-ammon, when the quantity of sulphuric acid in it was to be determined. There were obtained 2·040 grammes of sulphate of barytes. The quantity of chloride of silver corresponds to 22·306 per cent. of chlorine, and the quantity of sulphate of barytes to 20·35 per cent. of sulphur in the compound.

These quantities answer to a compound, which consists of one atom of sulphate of chloride of sulphur, and of eighteen simple, or nine double atoms of ammonia ( $\text{S Cl}^{\text{p}} \text{S}^{\text{s}} + 9 \text{NH}^{\text{s}}$ ). Per hundred it is constructed thus:—

Chlorine.....	22·23
Sulphur .....	20·26
Oxygen .....	25·15
Ammonia .....	32·36

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100·00

From 1·170 gramme of the compound, prepared at another time, were obtained, by the same method of treatment, 1·052 gramme of chloride of silver, and 1·733 gramme of sulphate of barytes, which answers to 22·18 per cent. of chlorine, and 20·44 per cent. of sulphur in the compound, which agrees with the results of the first analysis, as well as with the calculated composition.

The compound is constituted exactly as must have been expected from what had previously been stated and understood. If the chloride of sulphur ( $\text{S Cl}^{\text{p}}$ ) could be exhibited in an insulated state, it would, as is highly probable, and as I have before observed, take up so much ammonia, that when the compound was treated with water, it might be considered as a compound of chloride of ammonium and of sulphate of oxide of ammonium; or, more correctly, perhaps, of sulphat-ammon. With this view, one atom of the chloride would have four double atoms of ammonia. Since, now, the five atoms of an hydrous sulphuric acid, which are contained in one atom of sulphate of chloride of sulphur take up five double atoms of ammonia to form sulphat-ammon, so must one atom of sulphate of chloride of sulphur combine with nine double atoms of ammonia.

*Regnault*\* has examined the compound, first exhibited by himself, of ammonia, and sulphate of chloride of sulphur; which, analogous to the chromate of chloride of chromium, consists of 2 atoms of sulphuric acid and 1 atom of the chloride of sulphur, ( $\text{S Cl}^{\text{p}} + 2 \text{S}$ ). He has found that this compound takes up 6 double atoms of ammonia, which is also exactly the quantity which, in the ammoniacal compound, might be expected. It differs essentially in other respects from that which I had previously prepared, as it deliquesces in the air, which is not the case with the other.

But *Regnault* regards neither the sulphate of chloride of sulphur, nor the compound of that substance with ammonia, in the manner as is done in this memoir. Owing to the substitution-theory of

\* *Annales de Chemie et Physique*, tom lxxix. p. 176.

*Dumas*, and the views which *Walter* and *Persoz* have taken, on the constitution of the chromate of chloride of chromium and similar compounds, he considers the compound ( $\text{S Cl}^3 + 2 \text{S}$ ) as a sulphuric acid, in which one-third of the oxygen is replaced by chlorine, therefore as  $\text{S Cl}$ . The compound of this with ammonia is, according to him, a mixture of a sulph-amid  $\text{S N H}^2$  (analogous to the oxamid), and of chloride of ammonium.

As regards the first view, I have already, on a former occasion, endeavoured to shew, that the combination prepared by me ( $\text{S Cl}^3 + 5 \text{S}$ ) cannot well be considered the same as ( $\text{S} + 2\frac{1}{2}\text{O} + \text{Cl}$ ), and the reasons which I therefore mentioned make it also probable that *Regnault's* chloro-sulphuric acid ought to be considered as a sulphate of chloride of sulphur. I shall, however, in another part of this memoir, give other reasons for the support of the views I have taken.

As regards *Regnault's* view of the nature of the ammoniacal compound, he admits himself, that it was impossible to separate the chloride of ammonium from the sulph-amid with which it was mixed; as both bodies, as he supposes, have nearly the same solubility in both water and alcohol, they could but very imperfectly be separated by crystallization.

The only reason which *Regnault* has given in support of the view that the ammoniacal compound contains chloride of ammonium is, that from its solution the whole of the chlorine can be thrown down as chloride of silver, by a solution of nitrate of silver; and further supposes, that chloride of platinum precipitates only that portion of the ammonia from the solution which it contains as chloride of ammonium, and *not* the amid, in the hypothetically adopted sulph-amid.

From all compounds of volatile chlorides with ammonia, that I have examined, the amount of chlorine, after their solution in water, may be determined in the usual manner by nitrate of silver, whether they contain more or less ammonia than is requisite to form chloride of ammonium with the quantity of chlorine. All these ammoniacal compounds, at least, all those that I have examined, have also the property of retaining a portion of ammonia, so that the whole amount cannot be precipitated; frequently, indeed, by chloride of platinum, only about one-half can be precipitated, which separates from their solutions as chloride of ammonium and platinum. It appears as if the oxyde of ammonium only, and not the ammonia, were susceptible of precipitation by chloride of platinum; for although the sulphat-ammon, as well as para-sulphat-ammon, in both of which ammonia only is contained, yield, after their solution in water, precipitates with chloride of platinum, still the greatest portion of the ammonia is not precipitated by that treatment. If the liquids filtered from the precipitate be evaporated, and hydrate of potash be added, they still give a

strong ammoniacal smell. It is true that carbonat-ammon (anhydrous carbonate of ammonia), gives a solution in water from which chloride of platinum precipitates the whole of the ammonia;\* but this is a proof that the carbonat-ammon, by its solution in water, is transposed into carbonate of the oxyde of ammonium. The aqueous solution of carbonat-ammon behaves itself in a similar manner to the solutions of the carbonates of the oxydes of ammonium. The carbonic acid in the solution has not lost its susceptibility of being recognised by the application of those re-agents which detect it in other compounds. This is also the case with sulphuric acid in the sulphat and para-sulphat-ammon, which, by water, are not transformed into salts of the oxyde of ammonium.

I have instituted several experiments in order to ascertain whether or no chloride of ammonium, mixed with a sulph-amid, be contained in the compound of the sulphate of chloride of sulphur with ammonia, as prepared by myself, or whether it be a peculiar compound. The results of all the experiments are decidedly in favour of the latter view. I will place but little importance upon the curious constitution which such a sulph-amid must have; notwithstanding, however, it is deserving of being mentioned. If we suppose, according to the theory of substitution (*substitutions theorie*), that the constitution of the sulphate of the chloride of sulphur agrees to the formula  $S + 2\frac{1}{2} O + Cl$ , or rather to  $S^2 O^5 Cl$ , then the latter must take up  $3 N H^3$  to form the ammonical compound. If in such a chloride of ammonium there be mixed a sulph-amid, then again the latter may be a mixture of a peculiar sulph-amid with sulphat-ammon. For,  $S^2 O^4 Cl + 3 N H^3 = Cl N H^4 + S^2 O^4 N^3 H^3$ ; but the latter compound must be regarded as  $\bar{S} N H^3 + \bar{S} N H^3$ . The ammoniacal compound would then be a mixture of three distinct compounds, viz. of chloride of ammonium, of sulphat-ammon, and of sulph-amid.

I have dissolved a considerable quantity of the ammoniacal compound in water, and evaporated the solution over sulphuric acid in the receiver of an air-pump. Crystalline films are formed by this process, but it was not possible to distinguish any diversity in the form of the crystals. They appeared as homogeneous, but, in reality their exact forms could not be determined. I expected to obtain crystals of para-sulphat-ammon, but even these were not discerned at any period of the evaporating process. The evaporated mass appeared, for a long time, moist and smeary, but eventually dried up completely. It was left in vacuo till it no longer decreased in weight.

1.227 gramme of the dried mass, examined in the above manner, gave 1.136 gramme of chloride of silver, and 1.794 gramme of sulphate of barytes. This corresponds to 22.84 per cent. of chlorine, and 20.17 per cent. of sulphur, in the compound.

\* Poggendorff's Annalen, bd. xlv. s. 361.

The compound, dissolved in water and evaporated, is composed in precisely the same manner as that originally prepared : so that none of the water is taken up by this treatment.

It results from these researches, that the ammoniacal compound is not a mixture of several substances, but a compound of a peculiar kind. If, however, that be the case, the constitution of this compound leads to some not unimportant considerations.

According to *Walter* and *Persoz*, the recently discovered volatile compounds of acids with analogously constituted chlorides, are considered as acids in which the oxygen is replaced by chlorine. This supposition, which springs from the theory of substitution, at first sight seems very plausible, and recommends itself by its simplicity; for although oxygen, as regards its properties, stands tolerably insulated, has of all elements the greatest similarity to sulphur on one hand, and on the other, to chlorine or to fluorine; it would, therefore, it is true, be a highly interesting fact, though to many chemists, perhaps, not an unexpected one, were an isomorphous compound to be discovered, in which an equivalent of chlorine holds the position of oxygen. Immediately we come to a closer consideration, however, the probability of this view begins to vanish. If we compare the combinations which oxygen and chlorine form with hydrogen in their gaseous state, we find that they possess quite different relations of condensation. (*Verdichtungsverhältnisse.*) The condensation relations of gaseous compounds, which contain elements that can substitute each other in other solid compounds, and which to one another are isomorphous, are in other respects also always the same. Such is the case in compounds of chlorine, bromine, and iodine, with hydrogen; and also with phosphorus and arsenic with the same element.

*Berzelius* also speaks decidedly, from other grounds, against the view, that chlorine can substitute oxygen in the so-called basic hydrochlorate salts, or in the compounds of acids with chlorides in question; and, relative to the latter compound, he retains the view which I disclosed when I made known the constitution of the chromate of the chloride of chromium.\*

If we combine two isomorphous acids with one base, or two bases with one acid, so are there always similar atoms of base or of acid which are taken up by the isomorphous substances. And if we were to mix the isomorphous acids and isomorphous bases in any proportion, the quantity of base or of acid which would combine with the mixture, would always stand in a similar relation as the quantities which had been combined with the acids or bases separately.

Amongst the bases, ammonia is of a peculiar kind. It appears as a distinct base, however, only when it has not taken up water or its constituents; for its association with those bodies converts it

\* *Annalen der Pharmacie*, bd. xxxi. s. 113.

into an oxyde of ammonium, which is a base perfectly analogous to the other oxy-bases. In the compounds of ammonia with acids (the ammoniums), when treated with water, the ammonia either remains as such with the acid combined, as in sulphat-ammon and para-sulphat-ammon; or it becomes converted into oxyde of ammonium, as in carbonat-ammon and other ammoniums which I have recently had occasion to examine.

When two acids, which with bases give isomorphous compounds, combine with ammonia to form neutral ammoniums, it is highly probable that each acid takes up a similar number of atoms of ammonia. Even when both acids have been mixed in different proportions, the ammonia taken up by this mixture must stand to it in the same proportions as to the acids individually.

If, for instance, in the anhydrous sulphuric acid, a portion of the sulphur were to be substituted by selenium, or even by chromium, the new acid, which might be considered as a compound of selenic acid, or of chromic acid with sulphuric acid, would be able to take up just the same number of atoms of ammonia as the latter acid alone. But the same should also be the case when, in the sulphuric acid, the other element, oxygen, is substituted by chlorine, in a similar manner as the sulphur by selenium or by chromium.

But the results of the above-stated experiments show that such is not the case. Both *Regnault's* compound, and the sulphate of chloride of sulphur, which I have prepared, take up more ammonia than if they were sulphuric acids in which a part of the oxygen were substituted by chlorine.

*Regnault's* compound is  $\text{S Cl}^3 + 2 \text{S}$ ; or, according to his view,  $\text{S Cl}$ . Were we to regard it as a sulphuric acid, in which a part of the oxygen is substituted by chlorine, and in which the latter completely occupies the place of the oxygen; then, as one atom of sulphuric acid takes up one atom of ammonia to form sulphat-ammon,  $\text{S Cl}^3 + 2 \text{S}$  should take up three double atoms, and  $\text{S Cl}$ , one double atom, of ammonia. But according to *Regnault's* own researches, there are taken up by the compound, in the first case *six*, and in the second *two*, double atoms of ammonia: therefore twice as much as, according to the substitution theory, would be expected.

The compound of chloride of sulphur with sulphuric acid, which I have prepared, is  $\text{S Cl}^3 + 5 \text{S}$ . Accordingly, with the substitution theory, we should consider this compound as  $\text{S} + 2\frac{1}{2} \text{O} + \text{Cl}$ ; or rather as  $\text{S}^2 \text{O}^5 \text{Cl}$ . In the first case, then, accordingly with this theory, it ought to combine with *six*, in the second case with *one*, and in the third case with *two*, double atoms of ammonia. But the experiments have shewn that, in the first case *nine*, in the second case *one and a half*, and in the third case *three*, double atoms of ammonia are taken up by it. I believe we may hence conclude, that all the volatile chloro-compounds which I for some time past have looked upon as compounds of oxygen and chlorine, must still



be considered in that light, and not as acids in which a portion of the oxygen is substituted for chlorine.

*Liebig* has advanced a weighty reason in support of the substitution theory.\* He draws attention to *Mitscherlich's* discovered isomorphism of the hyper-chlorates and hyper-manganates, from which it appears that chlorine may be substituted by manganese. But the fact is, that chlorine and manganese have a much less similarity to each other than chlorine and oxygen. Nevertheless, if even no similarity can be found in the elements, manganese is as decidedly a radical as is chlorine. Both can combine with oxygen, and in the highest states of oxydation there is a remarkable analogy between them. A similar difference is exhibited by sulphur and chromium, which can substitute each other, and several sulphate and chromate salts are isomorphous. Hence it follows, that also a substitution of sulphur, and of chromium, by chlorine, may possibly take place.

When, however, a substitution of oxygen by chlorine cannot be admitted, so is a substitution of hydrogen by chlorine still less probable. But, as is well known, this substitution is the most peculiar feature in the theory of *Dumas*; and a great number of cases appear to render it probable, although in numerous other cases this substitution has no place.

No view, perhaps, in modern times, has been so stimulating and fruitful as that of *Dumas*, on the substitution of hydrogen by chlorine. We are indebted to it for an extensive series of excellent researches in organic chemistry, which, without this excitement, might not have been attempted. But all these labours do not shew that this substitution is incontrovertibly correct, if we except the most recently discovered isomorphism of oxamethan and chloroxamethan, by *De la Provostaye*.†

Although there may be a great number of substitutions which cannot be known by isomorphism, yet, without contradiction, this is the surest indication of a true substitution. Therefore, after the discovery of *De la Provostaye*, no objection could be made to the substitution of hydrogen by chlorine, if this discovery were confirmed. But from the given description of the forms of the crystals, the isomorphism cannot be relied on.

Should, however, the substitution of hydrogen by chlorine be incontrovertibly shewn by many examples, by the isomorphism of the compounds which these elements form with one another, we must then, without hesitation, admit the conclusion, that in complex compounds the grouping of the atoms, and not the different chemical nature of the elements, occasions the analogy in the attributes.

The same conclusions can be drawn from the compound of sulphate of chloride of sulphur with ammonia; and follows, also, from the compound of carbonate of chloride of carbon (*phosgengas*,

\* *Annalen der Pharmacie*, bd. xxxi. s. 119.

† *Annales de Chimie et de Physique*. T. lxxv, p. 322.

phosgene gas) with ammonia. The carbonate of the chloride of carbon  $\text{C Cl}^2 + \text{C}$ , according to the substitution theory, will be regarded as a carbonic acid, in which half of the oxygen is substituted by an equivalent of chlorine,  $\text{C Cl}$ . But one atom  $\text{C Cl}$  takes up two double atoms, or  $\text{C Cl}^2 + \text{C}$  takes up four double atoms of ammonia. The anhydrous carbonic acid, however, can combine only with one double atom of ammonia, to form carbonat-ammon,  $\text{C} + \text{N H}^2$ , when even the carbonic acid gas is mixed with the greatest excess of ammoniacal gas. Since, however, the compound contains but just twice as much ammonia as, according to the substitution theory, it ought to do, it follows, from its composition, also, that in the carbonate of chloride of carbon, the chlorine cannot be looked upon as substituting oxygen.

The carbonate of chloride of carbon fixes exactly an analogous quantity of ammonia as the sulphate of chloride of sulphur. Were the compound to be treated with water, its constitution might be considered as a compound of chloride of ammonium and carbonat-ammon. *Regnault* views it as a mixture of chloride of ammonium with a carb-amid,  $\text{C N H}^2$ , without, however, having accomplished a separation of its constituents.\*

*Experiments on the Magnetic Powers of Attraction and Repulsion, at various Distances; and on the Relation between those Powers and the immediate Lifting Powers of Magnets.* By Professor CRAMER, of the University of Mechanics, KIEL.†

In these experiments I have employed a very accurate, so called, weigh-beam, whose scales, during their vertical motions, always remained in an exact parallel position; and that, although the instrument is capable of weighing 500 lbs., a half ounce weight will move the scales about a line, or the twelfth part of an inch, when they are loaded with no more than about 10 lbs. To one of the scales, which rests on a two-inch-board, I fasten, side ways, a horse-shoe magnet, whose poles are afterwards turned upwards. After the equilibrium was restored by placing weights in the opposite scale, another magnet, similar to the former, was fixed into a solid stand, and so situated that the dissimilar poles of the two magnets could be brought into close contact with one another. After satisfying myself of the accuracy of the arrangement, it was very easy to ascertain the lifting power of the magnets, by carefully and gradually placing weights in the scale to which the lower magnet was fixed. In repetitions of the experiments, however, there is to be considered that, when the poles have become once separated, the maximum of

\* *Annales de Chimie et de Physique*, tom lxi. p. 180.

† *Poggendorff's Annalen der Physik und Chemie*. No. 2. 1841.

weights which they accurately carried, cannot immediately be carried again; but that, in order to restore the same power as at first, it is necessary that the poles be a short time, about a minute, in contact.

In order to ascertain the powers of magnetic attraction at different distances, I instituted the following series of experiments, in which, as an unit of distance, I employed the thickness of a sheet of vellum, or post paper, which is about one forty-sixth of an English line, or one four-hundred-and-sixtieth part of an inch thick. I furnished myself with a number of pieces of this kind of paper, and placed them, gradually increasing the number, between the poles; so that by this means I obtained a series of distances of 2, 3, 4, 5, &c., papers thick.

In this manner I have proceeded with four series of experiments: the first, with two small magnets of a lifting power of 104 ounces; the second, with two larger magnets of a lifting power of 224 ounces; the third, with one magnet of 680 ounces lifting power, with a soft iron cross-piece (Anker); and, the fourth, with a small magnet of 80 ounces lifting power, also with a cross-piece of soft iron.

We may easily understand, that the paper, by the very different degrees of pressure which it would suffer, would lose more or less of its original thickness, which may appear sufficient to produce very important discrepancies; but, as to myself, I am convinced to the contrary, in so far as I submitted the paper to a pressure of 20 lbs., and, when again employed, the lifting power was still the same as before; so that we may consider the paper as completely elastic after this pressure as before. Even in the thickness of the different sheets of paper employed, I have not been able to find any remarkable difference in the results of the experiments. In the following tables I have placed the results of the four series:—

LIFTING POWER = 104 OUNCES.

Distance.	Power of Attraction.	Difference.	Distance.	Power of Attraction.	Difference.
1	44 oz.	..	9	4 oz.	1
2	24 "	20	10	3½ "	2
3	16 "	8	11	3 "	3
4	11 "	5	12	2¾ "	4
5	8½ "	2½	13	2½ "	5
6	7 "	1½	14	2¼ "	6
7	5½ "	1½	15	2 "	7
8	4½ "	1½	..	..	..

LIFTING POWER = 224 OUNCES.

Distance.	Power of Attraction.	Difference.	Distance.	Power of Attraction.	Difference.
1	148 oz.		15	$18\frac{1}{2}$	$1\frac{3}{8}$
2	108 "	40	16	$17\frac{1}{4}$	$1\frac{5}{8}$
3	84 "	24	17	$15\frac{7}{8}$	$1\frac{7}{8}$
4	64 "	20	18	$15\frac{1}{4}$	$1\frac{9}{8}$
5	54 "	10	19	$14\frac{5}{8}$	$1\frac{11}{8}$
6	45 "	9	20	14	$1\frac{13}{8}$
7	40 "	5	21	$13\frac{3}{8}$	$1\frac{15}{8}$
8	$36\frac{1}{2}$ "	$5\frac{1}{2}$	22	$12\frac{7}{8}$	$1\frac{17}{8}$
9	31 "	$5\frac{1}{2}$	23	$12\frac{3}{8}$	$1\frac{19}{8}$
10	28 "	3	..	..	..
11	25 "	3	..	..	..
12	23 "	2	..	..	..
13	$20\frac{5}{8}$ "	$2\frac{3}{8}$	..	..	..
14	$19\frac{1}{2}$ "	$1\frac{1}{8}$	$46=\frac{1}{10}$ in.	$6\frac{1}{2}$	..

LIFTING POWER = 680 OUNCES.

Distance.	Power of Attraction.	Difference.	Distance.	Power of Attraction.	Difference.
1	475 oz.	..	15	60 oz.	$4\frac{1}{2}$
2	355 "	120	16	$55\frac{1}{2}$ "	$4\frac{3}{2}$
3	267 "	88	17	$51\frac{1}{2}$ "	4
4	215 "	52	18	$47\frac{1}{4}$ "	$4\frac{1}{4}$
5	184 "	31	19	$44\frac{3}{4}$ "	$2\frac{3}{2}$
6	152 "	32	20	$41\frac{1}{4}$ "	$3\frac{1}{2}$
7	130 "	22	21	$39\frac{1}{4}$ "	2
8	116 "	14	22	$37\frac{1}{4}$ "	2
9	103 "	13	23	35 "	$2\frac{1}{4}$
10	92 "	11	..	..	..
11	84 "	8	..	..	..
12	76 "	8	..	..	..
13	70 "	6	$46=\frac{1}{10}$ in.	$13\frac{1}{2}$	..
14	$64\frac{1}{2}$ "	$5\frac{1}{2}$	..	..	..

LIFTING POWER = 80 OUNCES.

Distance.	Power of Attraction.	Difference.	Distance.	Power of Attraction.	Difference.
1	32 oz.	..	9	$3\frac{1}{2}$ oz.	1
2	21 "	11	10	$2\frac{7}{8}$ "	$\frac{5}{8}$
3	14 "	7	11	$2\frac{1}{2}$ "	$\frac{3}{8}$
4	11 "	3	12	2 "	$\frac{1}{8}$
5	$8\frac{1}{2}$ "	$2\frac{1}{2}$	13	$1\frac{5}{8}$ "	$\frac{3}{8}$
6	$6\frac{1}{2}$ "	2	14	$1\frac{3}{8}$ "	$\frac{1}{4}$
7	$5\frac{1}{4}$ "	$1\frac{1}{4}$	15	$1\frac{1}{8}$ "	$\frac{1}{4}$
8	$4\frac{1}{4}$ "	1	..	..	..

By looking at these series of experiments, we readily discover, (1) that at a very small distance we lose a considerable portion of the lifting power; less, however, in proportion to the mass, by a great lifting power than by a small one.

2nd, That within the range of these experiments, at least, the magnetic power of attraction by no means diminishes in the inverse proportion of the square of the distance (*sehr natürlich*, P.);\* but rather, even in a less proportion than inversely as the distance itself.

There happens to be so little accordance in these series of experiments, and the results of each individual series are so irregular, that it appears not possible to derive any general law from them. This want of uniformity is, to me, quite as unexpected as difficult of explanation; because I cannot attribute it to any inaccuracy in the management of the experiments, which, in themselves are exceedingly simple, and in the prosecution of which all possible care was taken.

I have, finally, endeavoured to determine the repulsive power of magnets, by turning similar poles of the magnets towards one another, whilst proceeding with the first and second series of experiments.

I found the available power in that kind of action but very feeble. In the first case, where the lifting power was 104 ounces, the repulsion, when strongest, viz., at the distance of about  $\frac{1}{2}$  of a line, was only  $\frac{1}{8}$  of an ounce; and when the magnets were brought nearer they attracted each other. In the second case, with a lifting power nearly double that in the first, viz. 224 ounces, the repulsive power of the magnets was about seven times that in the first case, or  $\frac{7}{8}$  of an ounce; and it is true, at the distance of half a line attraction was immediately the consequence.

\* We could not help smiling at Poggendorff's laconic satire on this passage of our author (*sehr natürlich*) i. e. very naturally. Professor Cramer's expression is just the reverse of that which he meant.—EDIT.

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#### *Blasting Rocks under Water by means of the Galvanic Battery.\**

At page 221, vol. xii, of the last series of this Journal, we published an article by Professor Hare, describing an apparatus for the blasting of rocks by means of galvanic ignition; and it will be seen, by the subjoined letter, that Captain Paris, a well known engineer and architect, of Boston, has applied the proposed means with perfect success, in blasting rocks under water. In the article by Dr. Hare, Mr. Moses Shaw, of Nova Scotia, is mentioned as having first suggested the idea of igniting the powder in blasting rocks, by the aid of the electric fluid. That gentlemen had pursued the subject with much persevering industry, contending, at the same time, against pecuniary difficulties, and a want of

\* Journal of Franklin Institute.

those resources which science alone can supply, in the prosecution of such undertakings. He well merits, however, to have his name associated with those who have brought the matter to a successful issue.

EDIT. JOURN. FRANKL. INSTIT.

DEAR SIR,—Knowing the great interest you have always manifested in all engineering operations connected with the construction of public works, it affords me pleasure to communicate to you an account of the transactions within the past summer at this Navy Yard, in blasting rocks under water, by means of the galvanic battery.

The application of this means to purposes of blasting, is somewhat novel, as you are well aware, and the account of Colonel Pasley's experiments in England has given to the public the first notice of its being thus employed. Since the blowing up of the wreck of the *Royal George*, it has been successfully used in England in blasting rocks and clearing harbours, rivers, &c., from obstructions: it bids fair to entirely supersede the old methods of blasting, both in civil and military operations, especially in the latter, where it becomes a tremendous agent for the instantaneous explosion of mines, &c.

In the detailed accounts of the experiments tried by Col. Pasley, it appears that at first many difficulties were encountered; and the numerous failures seemed to forbid any hope of success in large operations, although the result of those on a smaller scale generally proved satisfactory. Perseverance, however, enabled the operators, after many trials, to render the explosion of the charge under water as certain as by the ordinary methods on dry land: and the subsequent success in blowing up sunken wrecks, &c., at the bottom of the Medway river, and at Spithead, proved the utility of the means, and amply compensated for the labour and expense incurred in the first attempts.

Our operations during the past season were confined chiefly to the construction of quay walls and the foundations of two launching ways, the whole of which were built of stone. The character of the bottom of the river where the work was laid, rendered blasting or other means necessary, before a proper surface for the foundation could be obtained; it was desirable to give it a slight inclination inwards, so that the face of each course of stone should lie somewhat higher than the inside, thus preserving a proper batter of the walls and rendering them perfectly secure. This bottom is a hard slate rock, and, with the exception of some level portions, extremely uneven, with slopes of almost every grade, generally in an outward direction from the shore. The depth of water in the line of the walls varies from fifteen to twenty feet at low water, and from twenty-five to thirty below the high tides. This depth of water, added to a strong and variable current, caused me to anticipate much difficulty and great expense in all operations below its surface.

But we were fortunately provided with a fine diving apparatus, consisting of a cast-iron diving bell and a powerful air-pump attached. This apparatus was worked from a vessel of strong construction and light draught, fitted expressly for the purpose. A system of signals and messengers was established for communication between the workmen in the bell and those on board the vessel; by these means every want was speedily made known and answered. Four workmen, divided in two gangs, were employed for working in the bell, which made four descents per day, occupying at each time two and a half hours, the two gangs alternately relieving each other. The bell was amply supplied with a constant stream of fresh air, and but two or three inches of water remained in it at its greatest depth, so that the men worked in a comfortable state, perfectly dry, and with no more difficulty of respiration than on dry land.

In deciding upon the best means for preparing the bottom for the reception of the foundation of the walls, I was greatly at a loss which to adopt. It appeared to me that in adopting the method practised by Col. Pasley, great expense and difficulty would be incurred; and as it did not appear that this method had been employed in blasting the solid rock at the bottom of a river, in any of his experiments, I was somewhat apprehensive of its utility for operations of this kind, and whether the cost would justify the trial. In order to satisfy myself with regard to the expense of an experiment with the galvanic battery, I applied to Mr. Daniel Davis, Jr., philosophical instrument maker, of Boston, for the necessary information, when I was convinced that a very trifling expense would procure such a trial as would satisfactorily decide the merits of the apparatus. Mr. Davis kindly assisted me in making the experiments which were tried at the Navy Yard at Charlestown, and I had the pleasure of witnessing the most satisfactory results, and without hesitation determined to apply the means to the work in hand.

The galvanic battery, which was constructed by Mr. Davis, was one of Doct. Hare's invention, of Philadelphia. It consists of two vessels or jars, each formed by two concentric cylinders of copper, admitting of a cylinder of zinc between. Two copper wires, termed the conducting wires, formed the medium by which the electrical fluid was communicated to the charge from the battery. These wires were closely wound with thread, in order to prevent their coming in contact with each other, and both tightly covered with tape, and afterwards served round with twine, thus forming a single coil. At each extremity of the coil the wires were separated for a few inches, like a fork. This form of the galvanic battery, termed by Doct. Hare the "Calorimeter," is the most simple and portable of any that I have seen; its power for blasting gunpowder may be increased to any required degree, either by enlarging the size of the jars or increasing their number. We had, in addition to this apparatus, a simple contrivance for proving the charges of powder, which is termed the "Electrometer."

The charges used in blasting consisted of various quantities of gunpowder, according to the effect required, from four ounces to a pound. They were inclosed in perfectly air-tight tin cannisters, the smallest being an inch and a quarter in diameter, and the diameter of the largest about two inches; the lengths of the cannisters were eight or nine inches. Two copper wires were introduced into the cannister about half way down, with the extremities connected by a fine platinum wire; the other ends of the wires projected twenty or twenty-five inches beyond the mouth of the cannister, which, after being filled with powder, was closed and effectually secured with a waterproof composition. It will be observed, in thus preparing the charges, that the whole is completely air and water tight, and that no vent to the powder remains, an advantage of which I shall further speak.

The operation of blasting is carried on in the following manner: The hole in the rock for the reception of the charge is drilled to a proper depth by the workmen in the bell; the cannister is then inserted with the ends of the copper wires extending outside of the hole, which is then filled up or tamped with coarse sand. The ends of the conducting wires are then connected by means of clamps to the wires leading from the charge; the other end of the coil is then led up, as the bell is hoisted to the surface, to the battery, which in all our experiments was placed on a floating stage directly over the charge. The jars forming the battery are brought near each other, and their whole power concentrated by connecting them together with a short copper wire; the end of one of the conducting wires is then brought in contact with one pole of the battery, and the end of the remaining wire similarly disposed with the other pole, when the explosion instantly follows by the platinum wire in the charge becoming intensely heated as the electrical current passes through the conducting wires.

We made, during the past season, nine blasts, with but one failure, which was caused by the platinum wire in the charge becoming accidentally broken, so as to render the electrical circle incomplete; this probably occurred in tamping, an operation which must be conducted with care, as this accident is most liable to be incurred of all others, owing to the extreme delicacy of the wire. The object of the electrometer is to detect whether this has taken place before the charge is inserted in the rock, and may always be ascertained by a simple trial.

It must be obvious to every one, at all experienced in blasting rocks, that this method has advantages in many respects over the old methods, both under and out of water. The danger of accidental explosions is entirely prevented; these occur for the most part in the old practice by carelessness, while, in this, great care and nicety are required to produce the explosion. There is very little time required in charging, as the cannister is simply inserted in the hole and tamped with sand; the whole time occupied in this operation



and making the connection with the conducting wires in the present cases rarely exceeded twenty minutes. There is great expense and trouble saved in the absence of the train or fuse, which was indispensable in the old methods, especially under water, where was always required a water-tight hose or tube leading to the surface, which was always destroyed by the explosion. Here nothing is lost or injured except the cannister containing the charge. The explosion of the charge is reduced almost to certainty, and should cases of failure occur, it can be approached with safety, without the suspicion that fire may be near it. The most important advantage, in an economical view, is that the effect of the charges is much greater than in the old way, in consequence of there being no vent-hole; the whole explosive force of the powder is thus gained, while, by the old methods, much of it is lost. Our smallest charge displaced a much greater quantity of rock than the same amount of powder by the old means, which we had opportunities of experiencing. With these advantages, this method of blasting places in our hands the most ample means of clearing harbours and rivers of rocks, &c. in any reasonable depth of water.

In using Doct. Hare's apparatus, it appeared that an important advantage was gained over that of Professor Daniell, employed by Col. Pasley, inasmuch as a very troublesome arrangement, indispensable in the latter, was avoided. This consisted in not being obliged to insulate the conducting wires from the water, as in such a case the connection of the conducting wires with the charge must be made before the cannisters are placed in the rock; every portion, then, of the wires where the connection is made, must be covered with the waterproof composition. By Professor Daniell's apparatus, it appeared that water was a conductor, thus destroying the electrical circle, if any part of the conducting wires came in contact with it.

Though Doct. Hare's battery was known to Col. Pasley, it was not adopted in his experiments, the reason assigned being that "it did not appear that he had ever used it under water."

I have the honour, Sir, to be,

Your obedient servant,

ALEXANDER PARIS, Civil Engineer.

Col. S. THAYER, Boston.

Navy Yard, Portsmouth. N. H., Nov. 9, 1840.

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*An Account of the Establishment for the Manufacture of White Lead, from the Pulpy Oxyde of Lead, under the Patent granted to Mr. G. F. HAGNER.*

At page 274, of the last volume, we published an article "On the manufacture of white lead from the pulpy oxyde prepared by trituration,"\* in which we intimated that we expected to obtain some further information upon

\* See article on the manufacture of white lead, vol. v., p. 460, of these Annals.  
—EDIT.

this subject. The subjoined letter, from a gentleman who was a principal in the undertaking, affords this information ; and the character of the writer, for intelligence and integrity, is too well known to be strengthened by anything that we could say. Patents have been obtained, within a few years, by persons who appeared to consider the triturating process itself to be new, as described by them ; the facts which we have before published, together with those detailed in the accompanying letter, must put this question at rest ; and it will also appear that the proceeding in some other particulars was substantially the same with such as have been since claimed by others.—EDIT. JOURN. FRANK. INST.

*To the Editor of the Journal of the Franklin Institute.*

Shamokin, 12th Month, 21st, 1840.

RESPECTED FRIEND,—A succession of engagements have prevented an earlier reply to the inquiries made by thee respecting what I know relative to the discovery of making white lead by attrition ; I now offer, from recollection, the following facts, my papers relating to them being in Philadelphia.

I spent the winter of 1817-18 in London, and there met, for the first time, with Geo. F. Hagner, of the county of Philadelphia. He informed me that he had discovered a method of making white lead by attrition, that he had obtained a patent for his discovery in the United States, and that he had come to Europe for the purpose of procuring patents in such kingdoms as he might think it his interest so to do ; my belief is that he entered caveats, or took out patents, for England, Scotland, and Ireland. In the following spring he went to the continent, and remained there for some time.

About this period Jos. Richards, who had for several years been engaged in manufacturing white lead by the old process, built a mill, and put up machinery, at the Falls of Schuylkill, for making lead by attrition ; whether the plan pursued by him was identically the same as that patented and claimed by G. F. Hagner, I am unable to say, never having seen his works, the water power of his mill being destroyed, in common with all other power, at the Falls, by the dam at Fairmount. I did, however, see a portion of his machinery, which differed materially from that used and approved by G. F. Hagner.

Soon after I returned home from Europe, I met with G. F. Hagner, and was induced to invest a considerable sum of money in manufacturing white lead by his process. We commenced in the spring of 1820, in a small mill, on the west bank of the Schuylkill, below Flat Rock, but soon after removed to a mill built near the locks on the canal, at Manayunk. The first process was to melt the pig lead, and reduce it into very fine particles, as fine as gunpowder. The process by which this was done was cheap and rapid, but as G. F. Hagner never patented it, and kept it a secret from the

workmen, I do not feel at liberty to describe it ; I may, however, mention that the lead was not dropped into water, as stated in an article of the September No. of the Journal. The granulated lead was put into large cylinders made of wood, and lined with sheet lead. These cylinders were about six feet long, by four feet six inches wide at one end, and three feet at the other. The small end was open. They revolved on an axle passing through the centre. With the lead was put vinegar, or water, and as the liquor became thick with the white oxyde, it was taken out, and fresh lead and vinegar added. The white oxyde was allowed to settle ; was washed to rid it of the acid, then dried, and ground in oil, as usual. The article, as thus manufactured, had great specific gravity, covered well, and resisted the effects of sun and weather better than white lead made by the old process. It dried quickly, without the use of litharge, it was well adapted for outside use, but not for the interior of houses, as it became very yellow in confined rooms ; on account of this property it was not liked, and we expended considerable money to overcome this difficulty. In the course of our experiments I deemed it prudent to consult our late worthy and esteemed friend Wm. H. Keating, then Professor of Chemistry in the Faculty of Arts, in the University of Pennsylvania, who analyzed some of our white lead, and gave us his opinion respecting it. He had in his possession, at that period, an apparatus that had been used on the Brandywine, by a friend of his, in trying a set of experiments to produce the same result that we were trying to effect. This apparatus he lent us, and we used it at Manayunk.

After having spent many months, and undergone much labour of body and mind, and having also expended a considerable sum of money, we believed that we had arrived at a mode by which we could make a perfect article ; but as we had not room enough to operate in the part of the mill we occupied, I sold out at Manayunk, and erected buildings at Norristown adapted to our purpose, in which we could make two tons of white lead in every twenty-four hours.

In this new establishment we put up the large cylinders before described, but did not depend alone on the white oxyde made in them ; all that was made in these was used by us ; but our principal dependence was upon litharge. The litharge, or the white pulpy oxyde, either together or separate, was run through a pair of mill stones, with distilled vinegar, and in this liquid state the ground material was passed through a set of tubes or cylinders connected by copper pipes. These cylinders were air-tight, lay on their sides, and were fitted up with dashers much like a common butter churn. With the litharge and vinegar, was forced into the cylinder a large quantity of carbonic acid gas. These materials passed through eight cylinders, with dashers, and into a ninth, without dashers ; from this last cylinder the gas passed into the open air through a high copper tube, and the liquid into tubs placed on the floor, so as to admit of the settling of the lead ; these tubs were connected with tubes

which allowed the vinegar to pass into a reservoir, to be used over again.

The lead was then washed to get clear of the acid, dried, and ground, as usual. The white lead thus made was a perfectly carbonated article, very fine and white. Its specific gravity was less than that of the best white lead made on the old plan, and hence there arose an opinion, in the mind of the consumer, that it was adulterated. At the exhibition of the Franklin Institute, in the year 1826, I obtained the premium, in competition with the best lead there. The article we made was beautiful, and the arrangement as complete as any I ever saw.

In justice to Philip Mayer, a German, of considerable chemical and mechanical knowledge, I should say, we had his aid in this establishment.

In conclusion, I may state, we had the result of upwards of two thousand experiments, tried during a period of five years, and that finally we succeeded so far as to produce a truly excellent article. To give a description of our numerous experiments, would fill a number of the Journal.

In this letter I have endeavoured to give the kind of information which I believe was desired by thee; if not, please inform me, and I shall be glad to afford thee any other in my power.

I am, very respectfully, thy friend,

SAMUEL R. WOOD.

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In the foregoing letter, the writer mentions that the lead was granulated by a process which Mr. Hagner did not communicate to his workmen, and which he, therefore, does not feel himself authorised to disclose; we, however, are at liberty to offer our conjectures respecting the manner in which the granulation spoken of was effected, and which, we suppose, was upon a principle well understood by chemists and others. We apprehend that the lead, in a fused state, was submitted to continued agitation in a revolving iron cylinder, or other suitable vessel, the agitation being continued until the metal had set, or lost its fluidity, when it would be found in the state of fine grains, as above described.—EDIT. JOURN. FRANK. INST.

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*An Account of Researches in Electro-Chemistry.* By Professor SCHÖNBEIN, of Basle.\*

The British Association for the Advancement of Science, at their last meeting in Birmingham, honoured me with the charge to undertake a series of experiments, with the view of extending the limits of our knowledge on the connection which is supposed to exist between electrical and chemical phenomena. The memoir which I now take the liberty to lay before the association, contains

\* Reports of the British Association for the Advancement of Science.

an account of the results of my late investigations, many of which may perhaps appear as not immediately bearing upon the subject in question; but I think them, nevertheless, closely connected with it. I must, however, not omit to say, that my task is still very far from being accomplished.

It is familiarly known, that a peculiar odour, resembling that of phosphorus, is developed whenever common electricity passes from metallic or any other conducting points into atmospheric air; but I am not aware of having seen it anywhere stated, that a similar odour is disengaged during the electrolysis of water, though there can be no doubt that, besides myself, more than one philosopher has observed that phenomenon. The complete ignorance in which we still remain of the true cause of the electrical smell, and of the appearance of the latter under circumstances apparently so different from each other, cannot fail to excite scientific curiosity to a high degree, and stimulate philosophers to employ all their experimental means and mental powers to clear up the mysterious phenomenon. My own endeavours to solve the problem have been manifold, and for a long time were fruitless; at last, however, I succeeded in ascertaining some facts which promise to throw light upon the subject in question. Respecting the disengagement of the electrical odour during the electrolysis of water, as well as during the passage of common electricity from points into atmospheric air, my researches have led to the following results:—

1. The peculiar smell makes its appearance as soon as the electrolysis of water begins, and continues to be perceived for some time after water has ceased to be decomposed.

2. The phosphorus smell is produced at the positive electrode only, and under no circumstances whatsoever at the negative one; for when the gases resulting from the electrolysis of water are received in separate vessels, the smell is perceived only in that which contains oxygen.

3. The odoriferous principle can be preserved in well-closed vessels for a great length of time, whether mixed with oxygen or with detonating gas.

4. The disengagement of the smelling substance depends—

- (a) Upon the nature of the positive electrode.

- (b) Upon the chemical constitution of the electrolytic fluid, and

- (c) Upon the temperature of that fluid.

With regard to the circumstance mentioned under (a), my experiments have shewn that it is only well-cleaned gold and platina which are capable of disengaging the odoriferous principle. The more readily oxydable metals, as well as charcoal, do not possess that property at all.

It is worthy of remark, that iron, though acting (agreeably to my former experiments) like gold and platina when performing the function of the positive electrode, does not permit the disengagement of the odour.

As to the condition mentioned under (b), I have ascertained that the odoriferous principle is obtained from distilled or common water when mixed with chemically pure, or with common sulphuric acid, with phosphoric acid, nitric acid, potash, and a series of oxy-salts. I could not get a trace of it from aqueous solutions of chlorides, bromides, iodides, fluorides, hydro-chloric acid, hydro-bromic acid, hydriodic acid, hydro-fluoric acid, sulphate of protoxyde of iron. If to the fluids (above mentioned) which permit the disengagement of the peculiar smell, small quantities of nitrous acid, iron vitriol, proto-chloride of iron or of tin are added, not the least portion of the odoriferous principle will be given out, however actively water may be electrolysed. With regard to the aqueous solution of potash, I have observed the curious fact, that the smell is sometimes disengaged from it and sometimes not, the latter case occurring much more frequently than the former. I do not know yet the cause of that anomaly.

Concerning the influence which temperature exerts upon the developement of the peculiar smell, I have found that a fluid from which the odoriferous principle is abundantly disengaged at a comparatively low temperature, does not yield a trace of it when heated near its boiling point. I must not omit to state, that dilute sulphuric acid is the fluid best fitted for producing the smelling substance, and making the experiments which this memoir refers to. It sometimes happens, however, that even in making use of that fluid, the disengagement of the odoriferous principle is either suddenly stopped or does not take place at all. In such cases, the surface of the positive gold or platina electrode is not pure, *i. e.* it is covered with some foreign substance; and to cause the re-appearance of the peculiar smell, it is necessary to clean that electrode, which, by my experience, is best done by washing it first with pure muriatic acid, and afterwards with distilled water.

5. If some pinches of powdered charcoal, iron-, tin-, zinc-, or lead-filings, or of powdered antimony, bismuth, and arsenic, or some drops of mercury are thrown into a bottle containing the odoriferous principle, (mixed with oxygen,) the peculiar smell disappears almost instantaneously. Iron and charcoal seem, however, to act more rapidly than the other substances mentioned do; gold and platina, when strongly heated, also destroy the smell. Small quantities of nitrous acid and aqueous solutions of proto-chloride of iron, sulphate of protoxyde of iron, and proto-chloride of tin, being put into a vessel containing our peculiar principle, do likewise instantaneously annihilate the phosphorous smell.

6. A gold or platina plate, after having been kept only for a few moments within a vessel containing the odoriferous principle, (mixed with oxygen,) appears to be negatively polarised. To excite that polar state in the metals mentioned, it is a condition, *sine qua non*—

(a) That the surface of the plate of either metal be absolutely clean and entirely free from moisture.

(b) That the temperature of the metal be comparatively low. Heated gold or platina does not assume the negative polar condition. The current produced by such a polarised metal is of so short a duration, that it may be considered as instantaneous. Among the metals more readily oxydable than gold or platina, it is only silver and copper that are rendered negative by being put into an atmosphere of the odoriferous principle; but the degree of polarity acquired by these metals is exceedingly slight.

7. Gold and platina having been polarised in the manner indicated, maintain their peculiar condition for some length of time when placed in common air. I have found plates which had been exposed to the atmosphere, at least for a couple of hours, still perceptibly negative.

8. A polarised stripe of gold or platina loses, almost instantaneously, its negative condition, when plunged into an atmosphere of hydrogen. If the metal is kept in hydrogen longer than just required for destroying its negative polarity, it assumes, according to my former experiments, a positive condition. It will hardly be necessary to mention, that heat also destroys the polarity in question.

9. Oxygen obtained by the electrolysis of water, and deprived of its peculiar smell by the means indicated under § 5, has altogether lost its power of rendering gold and platina negative, and is, in a voltaic point of view, as inactive as oxygen prepared in the usual way.

#### *Phænomena of Polarisation caused by common Electricity.*

10. A gold or platina plate, having a perfectly clean and dry surface, assumes negative polarity when exposed to the action of a positive electrical brush issuing from a metallic or any other conducting point. The longer the brush is playing upon the surface of the metal, the higher will be the degree of polarity acquired by the gold or platina. A small platina stripe, after having been exposed to the action of a brush produced by thirty turns of my electrical machine, deviated the needle of a delicate galvanometer by  $60^{\circ}$ ; sixty turns, under the same circumstances, caused a deviation of  $90^{\circ}$ . It seems that the nature of the metal which performs the function of a point of emission, also exerts some influence upon the degree of polarity acquired by gold and platina. *Cæteris paribus*, a point of emission consisting of gold caused a deviation of  $170^{\circ}$ , a point of brass only one of  $60^{\circ}$ . As to the metals, (more readily oxydable than gold or platina,) I have only succeeded with silver and copper in polarising them negatively by common electricity. The degree of polarity acquired by the last-mentioned metals is, however, also exceedingly slight.

11. The negative electrical brush produces exactly the same voltaic effects as the positive one does.

12. A platina plate, polarised either by the positive brush or by the negative one, loses its electro-motive power when plunged only for a few moments into an atmosphere of hydrogen. All the remarks made under § 8 also apply to the case in question.

13. If gold or platina plates are connected with the prime conductor of an electrical machine, *i. e.* made points of emission, they will not assume any polar condition, however long and lively a brush may have been issuing from them.

14. Heated or moistened gold or platina plates, cannot be polarised by the electrical brush.

15. If the points of emission are heated or moistened, the electrical brush issuing from them has no longer any polarising power. It was impossible to excite in gold or platina even the slightest degree of negative polarity by holding these metals ever so long against a brush issuing from heated or wetted points.

16. The electrical brush proceeding from heated or moistened points does not produce the well-known phosphorus smell. The easiest way of depriving the brush of its smell, is to cover the point of emission with a piece of moistened linen.

Now in what manner are we to account for the facts above stated, and what are the inferences to be drawn from them?

As to the smell being developed at the positive electrode during the electrolysis of water, we can hardly help drawing, from the experiments mentioned in the first section, any other conclusion than that it is due to some gaseous substance disengaged (conjointly with oxygen) from the electrolytic fluid by the decomposing power of the current. But what is the nature of that substance? Is it elementary or compound? With regard to its voltaic bearings, it exhibits the strongest analogy to chlorine and bromine; of which bodies I proved, some time ago, that they possess, to a high degree, the power of negatively polarising gold and platina. I have also formerly shown, that these metals lose again their negative polarity acquired under the influence of chlorine and bromine, when plunged into an atmosphere of hydrogen; there is, consequently, not the least doubt that, as to its electro-motive power, the odoriferous principle bears the closest resemblance to chlorine and bromine. Now, does not this great analogy between the voltaic properties of chlorine, bromine, and our smelling principle, speak in favour of the supposition, that these three substances belong to the same class of bodies, *i. e.* to those which Berzelius called "halogenia?" If we take into further consideration the facts, (a) that most metals destroy the peculiar smell, that is, combine with the odoriferous principle in a direct manner, and even at a low temperature; (b) that the said principle is not disengaged at the positive electrode, unless the latter be composed of gold or platina; that is to say, of an eminently electro-negative metal; (c) that the odoriferous body is not eliminated by the current, if the electrolytic fluid happens to contain a substance having a strong affinity for oxygen, for instance,



sulphate of protoxyde of iron ; and (*d*) that our peculiar principle is always set free at the positive electrode, and never at the negative one ; I say, if we duly consider all these facts, we can hardly help drawing from them the conclusion, that the odoriferous substance is a body very like chlorine or bromine. The odoriferous principle, however, may perhaps be nothing but a secondary result of the electrolytic action. Such is no doubt possible, and indeed it was the first view I took of the case. The following reasons, however, seem to speak against the correctness of such a supposition. As chemically pure water, (or what we take as such,) mixed with sulphuric, nitric, and phosphoric acids, with potash and many oxy-salts, yields the odoriferous principle, we must conclude that the latter proceeds from water, and not from the other substances. Now what secondary product does water allow to be formed at the positive electrode ? The oxygen being eliminated at the latter, might certainly combine with some water, and produce per-oxyde of hydrogen. But this compound is not gaseous at the common temperature, and its vapour is wholly inodorous. I have besides observed, that platina enveloped with a film of per-oxyde of hydrogen, is positive to common platina. From these facts it follows, that the odoriferous principle cannot be per-oxyde of hydrogen. Or does another, perhaps, exist, consisting of hydrogen and oxygen, and containing more of the latter element than the per-oxyde does ? Are, perhaps, even chlorine and bromine compounds of a similar description ? The per-oxydes of manganese, lead, and silver, exhibit the same voltaic properties as chlorine and bromine, both groups of bodies being eminently electro-negative. Such a strong analogy, does it not indicate a similarity as to their chemical constitution ? I do not venture to answer any of these questions. The present state of chemical science does not yet warrant us to speak of chlorine and bromine as of compounds, and I shall therefore consider the odoriferous principle as an elementary body, and call it "Ozone," on account of its strong smell.

Now if we take it for granted that ozone is an elementary substance, and knowing that it originates in water, we must conclude that this fluid is made up of two electrolytes, one consisting of hydrogen and oxygen, the other of ozone and some electro-positive body. When a current is made to pass through such a fluid, both electrolytic compounds are decomposed, their anions, oxygen and ozone, being evolved at the positive electrode, their cations at the negative one. As to these cations, we know well enough that one of them is hydrogen. But is ozone also, like oxygen, united with hydrogen ? All the experiments I have hitherto made, with the view of discovering in the gas evolved at the negative electrode something besides hydrogen, have led to negative results, a circumstance which seems to prove that ozone, as met with in water, is combined with hydrogen.

I do not, however, yet consider this point as definitively settled.

That (what we call) pure hydrogen is capable of rendering gold and platina electro-positive, seems to me so extraordinary and so important a fact with regard to the chemical theory of galvanism, that I cannot but recommend it to the full attention of philosophers. Why indeed should a piece of platina, being surrounded with a film of hydrogen, and voltaically associated with common platina, produce a current when plunged into pure water? Which is the chemical action that possibly can take place under such circumstances? According to the present state of our chemical knowledge, it is very difficult, if not impossible, to give a satisfactory answer to that question. Has, perhaps, all the hydrogen hitherto prepared not been chemically pure, and does it always contain a principle still more electro-positive than hydrogen itself, that is, having a greater affinity for oxygen than hydrogen has? Careful experiments alone can decide these important points.

The fact above stated, that ozone is not developed at the positive electrode, unless the latter be either of gold or of platina, is, in my opinion, very easily accounted for. These metals having, at the common temperature, very little affinity for ozone, do not combine with it, any more than they do with oxygen in the same circumstances, whilst the other metallic bodies readily unite with the odoriferous principle.\*

At a high temperature, gold and platina appear to be capable of combining with ozone;† and this property seems to account for the fact, that from heated dilute sulphuric acid, for instance, the smelling substance cannot be disengaged. It is, however, possible that ozone, in the moment of its being eliminated by the current, reacts upon the heated water, combining with the hydrogen of the latter.

The fact that no ozone is set free if the electrolytic fluid happens to be mixed with some readily oxydable substance, with iron-vitriol, for instance, seems to depend upon a decomposition of water, the oxygen of the latter uniting with the protoxyde of iron, and its hydrogen with ozone. The non-appearance of the latter is indeed very easily understood, if we suppose its action to be entirely analogous to that of chlorine and bromine.

Before proceeding to the discussion of other facts, I must say a few words on the polarising influence exerted by ozone upon gold and platina. In former papers I have endeavoured to prove, experimentally, that certain voltaic conditions which some metals (placed under given circumstances) seem to assume, are not to be considered as real modifications of those metals themselves, (a view still maintained by some philosophers,) but as changes proceeding only from certain substances being in some way or other deposited upon those metallic bodies. If, for instance, platina becomes positively polarised in an atmosphere of hydrogen, and negatively in one of chlorine or bromine, that metal itself does not undergo

\* See § 5.

† Ibid.

the least change with regard to its natural voltaic properties; and it is only the film of hydrogen or chlorine that surrounds the metal, which is to be considered as the seat of the electro-motive power, or as the cause of the polarity. Exactly the same remarks apply to the negative polarity which gold and platina seem to assume when put into an atmosphere of ozone. By a sort of capillary action that substance adheres to the metal, and the latter being voltaically associated with another piece of the same metal (in its natural state) and plunged into water, the latter will be acted upon by ozone just in the same manner as under similar circumstances it would be by chlorine or bromine. The film of ozone covering the platina will unite with the hydrogen of water, and produce by that action a current. This current will last until all the ozone adhering to the platina stripe unites with hydrogen, but the quantity of that principle being exceedingly small, the duration of the current cannot be long.

Having fully developed my views on the chlorine and bromine circuits in the *Philosophical Magazine*, (August, 1839,) and everything said there applying to an ozone arrangement, I have no occasion to enter into further details respecting this branch of the subject.

As to the depolarising action which an atmosphere of hydrogen exerts upon gold and platina when negatively polarised by ozone, it perhaps may depend upon the combination of the latter with hydrogen. In order to account for the disappearance of the polar state, it is, however, not necessary to presume the taking place of such an action. As platina becomes positively polarised by hydrogen, and negatively by ozone, it is obvious that the opposite voltaic actions of these two elements must, under certain circumstances, exactly balance each other.

It is manifest that the facts considered throw a new light upon what is usually termed negative voltaic polarisation. The results which I obtained some time ago on that subject are pretty generally known. They seemed to prove that the negative polar state assumed by the positive electrode in water (holding oxy-acids dissolved) is due to a film either of oxygen or of per-oxyde of hydrogen. To the same cause I was inclined to ascribe the negative polarity excited in that portion of acidulated water which is near to, or in contact with, a positive electrode. From the facts above stated, it now appears that, in both cases, the negative polarity results from the presence of ozone. Indeed, water charged with that principle is negative to pure water, whilst the same fluid holding oxygen dissolved is, in a voltaic point of view, inactive to pure water. Water mixed with some per-oxyde of hydrogen bears to common water the same voltaic relation as zinc does to copper. Without having recourse to actual electrolysis, we may indeed produce, by means of insulated ozone, all the phenomena of negative voltaic polarisation, a fact which seems to prove the correctness of the explanation offered of the phenomena in question.

I also suspect that the peculiar condition of iron, or its inactive state, has something to do with ozone; but for the present I cannot enter into that subject.

It is now time to speak of the phenomena of polarisation produced by the agency of the electrical brush. On comparing them with those called forth by ozone, we cannot but perceive a strong analogy between both series of phenomena. The very same metals which become negatively polarised by ozone, are brought into a similar state by the action of the electrical brush. The same conditions to be fulfilled, in order to polarise gold and platina by ozone, are likewise requisite to render these metals negative by the agency of common electricity. The negative polarity developed by ozone is destroyed by the same means by which we annihilate the negative polar state called forth by the brush. These facts are sufficient to counterbalance the conjecture, that the cause of the negative polarity is in both cases the same, *i. e.* that it is a film of ozone surrounding the metals. I have stated (§ 16) that the brush can easily be deprived of its peculiar smell, and that by so doing the former loses its polarising power. This fact clearly proves that it is not the electrical brush or discharge itself that excites in the metals exposed to its action the negative polar state, but the odoriferous principle accompanying the brush. This principle affecting our olfactory nerves precisely in the same manner, and also producing the same voltaic effects as ozone does, are we not entitled to infer that the smelling matter in the electrical brush and the odoriferous principle evolved at the positive electrode, are identical bodies? It appears to me that they are, and I do not, therefore, hesitate to ascribe the familiar electrical odour to ozone.

But how does it happen that ozone makes its appearance, whilst common electricity is passing from the points of a charged conductor into the atmosphere? To account for such a remarkable fact, we certainly must suppose that there is an electrolytic compound present in the air, as well as we suppose one to be contained in water, and that the electro-negative constituent, or the anion of that electrolyte, is our ozone. The passage of electricity from the points of charged bodies into the surrounding air, being in fact nothing but the act of the restoration of a broken electrical equilibrium, or a current, it is not difficult to conceive how ozone is set free near the points of emission. Our supposed electrolyte being present in the atmosphere, requires only to be placed within the circuit of such a current in order to be electrolysed, or to have its anion, ozone, separated from its cation. If all these suppositions be correct, it follows that the electrolysis of our ozonic compound will be most vigorous where the emission of electricity is most abundant, and that consequently, at such a spot, the strongest smell of ozone will be perceived, and platina or gold acquire the highest degree of negative polarity. Now experiments prove that such is really the case.

That the peculiar odour perceived when any terrestrial object is struck by lightning has something to do with ozone, cannot be doubted. We have been hitherto profoundly ignorant of the nature of that odour, and everything said and conjectured about it by ancient and modern philosophers, must, in my humble opinion, be considered as totally unfounded. We know the fact only, and nothing more. The smell produced by lightning is usually described as being either sulphureous or phosphorous. Twice in my life I had an opportunity to observe this odour, once in the church of my native place (Mezingen, in Wurtemberg,) many years ago; another time in my own house at Basle, only last summer. The second case being still fresh in my memory, I shall say a few words about it. The object struck by lightning was a small chapel situated on the middle of the bridge of the Rhine, and about 200 yards distant from my lodgings. Immediately after the stroke had taken place, not only my house, but also the houses of my neighbours, were filled with a bluish vapour, and a pungent smell was perceived. Six hours after the occurrence, I entered into a parlour which had not been opened all the day, and I could still perceive the peculiar odour. My testimony is certainly not wanted to establish the fact, that lightning always causes the disengagement of an odoriferous principle; but I think that, on account of the great mystery which is still hanging over that phenomenon, the number of observations and statements about it cannot be too much increased. The fact related offers, besides that peculiar interest, that the smell was perceived at a comparatively great distance from the object struck by lightning. As far as my observations go, they incline me to consider the odoriferous gaseous substance set at liberty by the agency of lightning as ozone. Lightning being the same phenomenon on a large scale as the electrical spark or brush is on a small one, and our supposed electrolytic compound penetrating the whole atmosphere, electrolysis must take place, and consequently ozone be disengaged to a considerable amount, as often as lightning crosses our atmospheric air. The assertion of some observers, that the odour is of a sulphureous kind, and the statement of others, comparing it with the smell of phosphorus, may easily be reconciled to each other; for I have remarked that ozone, when somewhat condensed, is rather pungent, whilst the same substance mixed up with a large quantity of air, possesses a phosphorous smell. It is well known that the generality of people call any pungent odour sulphureous. Hence, if it happens that the odoriferous principle set free by lightning reaches the observer in a condensed state, he will describe it as sulphureous, but like phosphorus when inhaled mixed up with a good deal of air. Hence it follows, that the nearer the observer happens to be placed to the spot where a stroke of lightning takes place, the more pungent will be the smell perceived by him. The property of platina to assume negative polarity in a medium containing free ozone, seems to offer an excellent means to ascertain

the presence of that principle in the atmosphere. It appears, therefore, to be desirable to make experiments on that subject, and to place, for that purpose, platina stripes (not being insulated) in elevated regions, particularly on days when thunder-storms are taking place.

Before closing this paper, I must not omit to put a question of some importance. Does the electrolytic compound mentioned exist in our atmosphere quite independent of its aqueous vapour, or is it (the electrolyte) carried into the air by the evaporation of water? It is a matter of course, that this question can only be answered by experiments, and I have not yet found time enough to make them. Supposing the electrolyte to be carried into the atmosphere by the evaporation of water, the electrical brush should not produce any smell when passing into absolutely dry air, and the quantity of ozone disengaged would, *cæteris paribus*, be proportional to the quantity of aqueous vapour present in the atmosphere; *i. e.* would depend upon the hygrometric state of the latter. It is hardly necessary to say, that problems of the highest scientific importance would be raised, in case it should turn out that ozone can be produced in dry air. Be that, however, as it may, the ubiquity of our electrolyte can hardly fail acting a most important part in the household of nature; and it is not impossible that the electrical phænomena taking place within our atmosphere, the real cause of which is still covered in darkness, are closely connected with the workings of our presumed compound.

The fact of philosophers having not yet had the slightest notion of the existence of such a body is, I presume, no argument against its existence. If we suppose the electrolyte in question to be a substance closely resembling water in its chemical and physical properties, and existing in the latter fluid as well as in the atmosphere, only in very small quantities, it is easily conceivable why its existence has not hitherto been observed. I readily allow, however, that many researches, many experiments, must still be made before we arrive at certain results, at complete certainty, regarding the subject of ozone. Convinced as I am of its great scientific importance, I shall not fail devoting all my leisure time to its close investigation, and to sifting a matter to the bottom which promises to yield so rich a harvest of results. I should feel indeed very proud, if the British Association would honour me with the charge to present them an account of my researches next year.

It would be not right if I did not expressly state, before finishing my paper, that I owe most of the results above mentioned to a pile constructed upon my friend Mr. Grove's principle, an arrangement which cannot be too highly thought of.

C. F. SCHÖENBEIN.

Bâle, August 2, 1840.

*On Bone and its Uses in the Arts.* By A. AIKIN, late Secretary of the Society of Arts, London.

Most animal bodies are composed of soft and hard parts; of the latter, some are hard only when of a certain thickness, but when thin are tough, and more or less flexible and elastic; such as the horns of all mammalia (except of the stag tribe;) the claws of the lion and tiger; the talons of the eagle; the horn of the rhinoceros; the coriaceous covering of tortoises and crocodiles; and the scales of fishes.

All these, by exposure to a gradually increasing heat, soften, enter into pasty fusion, give out the odour of burnt feathers, burn with jets of flame, and are consumed, leaving behind a very small proportion of earthy matter. 500 grains of horn leave not more than from 0.25 to 2 of phosphate of lime. Boiling water, after long action, takes up from most of them scarcely any quantity of soluble matter, but they are perfectly soluble in caustic alkali, and the solution gives, with acids, a curdy precipitate. They are considered, therefore, as composed of condensed membrane, or, in chemical language, of albumen.

Other hard parts are rigid, considerably harder than the former; when dry, and in many cases when wet, they are very slightly flexible or elastic; and when struck by a hammer, or when bent beyond their power of resistance, break short with a splintery surface. When exposed to a red heat, with access of air, the membranous or animal part is destroyed; but the earthy part remains in sufficiently abundant quantity to retain the external form, and generally the internal structure, of the entire substance, of which calcined bone and calcined oyster-shell are examples. The original hardness of these parts is owing to the abundance of earthy matter that enters into their composition. When such parts are on the outside of the body, they are called, in common language, shells, horns, teeth, according to their position, and the uses for which they seem intended. When they occur within the body, they are called bones.

If, however, we restrict the term bone to its common meaning, we shall exclude the horns of the stag kind, and the substance which forms the body of most teeth, both which are truly bone, and shall include some substances, such a cuttle-fish bone, which is truly shell.

It is, therefore, necessary to enter into a more minute examination and comparison of these hard substances, in order to ascertain which of them are shell and which bone; and, as the result of our inquiry, we shall probably find, with respect to this class of natural bodies as with many others, that although the two extremes of the series are readily enough distinguishable from each other, yet they

approach by such insensible intermediate gradations, as to render it impossible to say where the one begins and the other ends.

There is a class of shells comprising most of the univalves, which are harder than other shells, and when broken, present thick, parallel layers, the layers themselves having usually a finely fibrous structure at right angles to the external surface. These fibres may often be seen to be nothing more than the transverse section of thin transparent parallel lamellæ, which, when viewed on their broad surfaces, often exhibit the usual natural joints of calcareous spar. When such a fracture is viewed by the naked eye, it has a good deal the appearance of porcelain,—whence their name of porcelainous shells. When carefully cleansed from all remains of the animal which inhabited them, they give out scarcely any perceptible odour on being made red hot, though their colour becomes somewhat grey. When unaltered they dissolve in dilute acid with much effervescence of carbonic acid gas, and a few hardly appreciable gelatinous flocks remain undissolved. These latter, on being collected and washed, give out, when heated, a faint odour of burnt animal matter, and become black before they are consumed. By proper chemical tests the soluble part of the shell is proved to have been carbonate of lime or chalk, the particles of which were cemented together with a very minute portion of animal mucus.

Another class of shells is the nacreous, so called from the varying and iridescent colours that they exhibit, resembling those of nacre, or mother-of-pearl: this very substance being, indeed, only a part of a nacreous shell.

These, when heated in a crucible, give out the odour of burnt feathers, often with a perceptible smoke, become of a dark-grey colour; and when submitted in this state to the action of acids, there remains undissolved a notable quantity of charcoal. In the recent state they effervesce with weak acids: and when the calcareous matter has been removed, there remains a series of flexible, membranous, or semigelatinous lamellæ, lying parallel to one another, and representing the form of the entire shell. These lamellæ have sometimes a distinctly fibrous structure, parallel to the surface of the shell; and though quite flexible while moist, they shrivel on drying, and become hard like horn,—a substance to which they bear the greatest possible analogy. The nacreous shells, therefore, are always very finely lamellar in structure, and are represented by some as composed of alternate layers of membrane and carbonate of lime; but the more probable opinion is, that the calcareous matter is intimately mixed with the membrane, rather than distinct from it. These shells increase in size, in order to accommodate themselves to the growth of the animal, by the deposition of new and larger layers from within; and hence the external surface is covered by concentric furrows or wrinkles, marking the outer margin of each successive layer.

Between the two classes of shells that I have described are



others, the minute structure of which I am ignorant of, but which differ considerably in the proportion and condition of their membranous ingredient.

Thus it appears that all shells, how much soever they may differ from one another in structure, agree in containing carbonate of lime as their only earthy ingredient: and an animal substance, nearly resembling, if not identical with, horn or membrane, as their consolidating or agglutinating ingredient.

Exactly the same substances, namely, carbonate of lime and membrane, in various proportions, form the constituent materials of madrepores and other hard corals.

On examining the hard covering of aquatic crustaceous animals, such as the crabs and lobsters, we find, after the action of acids, that there remains a whitish, soft, elastic cartilage, which represents the original shape of the part, and that the acid solution not only contains lime that had been in the state of carbonate in the original shell or covering, but likewise phosphate of lime, although in smaller proportion than the carbonate. The presence of this earthy salt forms an essential difference in chemical composition between proper shell and the covering of the crustacea, which latter substance may thus be considered as holding an intermediate position between shell and bone.

Some of the corallines, chiefly those belonging to the genera *Gorgonia* and *Antipathes*, approach still nearer in chemical composition to bone; and, indeed, are hardly to be distinguished from it, their earthy part being phosphate of lime with only a small admixture of carbonate, their figure and structure being represented by dense membrane, and, when boiled, they give out a notable quantity of true jelly, which, like other kinds of animal jelly, has the property of forming a precipitate with infusion of galls or of oak-bark.

The proportion of membrane in these substances varies considerably, so that while one species almost exactly agrees in composition with the horn of the stag, others contain so much membrane in proportion to earthy matter, as to be nearly identical with the bone of the cartilaginous fishes.

If a piece of true bone, in an unaltered state, be put into weak acid, (muriatic acid, on the whole, is the best,) a moderate degree of effervescence will take place, showing the presence of some carbonate. By a continuance of this process for some days all effervescence and chemical action will cease; what remains undissolved will still represent the size and form of the original bone; but it will be semi-transparent, will exhibit a distinctly cellular structure, will be soft, flexible, and, to a certain degree, elastic. If, after being washed, it is boiled in water, it will be found to be in part soluble; and the solution, when boiled down to a proper consistence, will become viscid, and will gelatinize on cooling, and by drying will be brought to the state of hard glue. This jelly, when

again dissolved in water, will become curdy and will give a grey precipitate with nutgall, and will exhibit all the other physical and chemical properties of gelatine; the remaining portion, insoluble in water, will become hard and somewhat brittle by drying, will burn in the fire like a piece of horn, will dissolve in caustic, fixed alkali forming a saponaceous liquid, and will show all the other properties of albumen or membrane.

The acid in which the bone was first steeped will give an abundant white precipitate of phosphate of lime by means of caustic ammonia, and will give a much smaller precipitate of carbonate of lime by carbonate of ammonia. Thus, by the action of a few simple re-agents, the essential constituents of bone are demonstrated. In this summary I have taken no notice of the oil or fat which is contained in the internal bones of all mammiferous animals, because it seems to be by no means an essential part of bone; the horn of the stag and other animals of the same kind being entirely free from it. On this account it is that hartshorn jelly, made by boiling the shavings of stags' horn in water, is often recommended to persons of very weak digestion in preference to other jellies, as being absolutely free from oil; for, though hard fat is incapable of dissolving in jelly, yet the softer oily fats will combine with it in small proportion.

But, although it is impossible to draw any marked line of chemical distinction between true bone and the indurated membranous textures that I have already mentioned, yet the mode of their origin furnishes a real and very important difference.

Of the organization of coralline bodies, indeed, we know nothing; for scarcely any of them have been even superficially examined when alive, and, when dried, all trace of structure in the soft parts is completely obliterated.

But with regard to the production of shell, both in univalve and bivalve testacea, we are certain that it never, as such, forms a constituent part of the living animal. A viscid fluid is secreted by certain organs; and it is only when discharged from the body that it assumes the consistence and other characters of shell: therefore, although we may with perfect propriety speak of the structure of shell, as we speak of the structure (that is, of the mechanical arrangement of constituent particles) of a crystal, it would be a gross misapplication of terms to speak of the organisation of shell; this latter meaning such an arrangement as is compatible with and necessary for the performance of vital functions. Shell is essentially a dead body, or rather one which never was alive; for though naturalists and collectors well know the difference between what they call a dead shell and another, they mean by this expression merely to point out the difference between an empty shell and one the inhabitant of which was alive at the time of its capture.

The way in which the hard covering of the crustacea is annually formed (for these creatures change their shell every year) has not been sufficiently examined to ascertain whether it is at first a mere

exudation which hardens out of the body of the animal, or is an induration of the cuticle by the deposition in its pores of calcareous matter conveyed thither by proper secreting vessels. If the former is the case, the shell of the crustacea is analogous to that of the testacea; if the latter, it somewhat resembles bone in the mode of its formation.

With regard to bone itself, there is no doubt that it is as truly organised and vital as any other part of the body. As soon as the rudiments of a young animal can be distinguished before its birth, the place of the future bone is indicated by a soft or semi-fluid matter inclosed in a delicate membrane; by degrees both the membrane and the matter which it incloses become more dense and cartilaginous; opaque white spots then appear, which soon after are penetrated by vessels carrying red blood; the deposition of bone then begins, and at the same time the cartilage seems to be gradually replaced by membrane. The rudimental bone, which at first was solid, now begins, at least in the long bones, to exhibit an internal cavity or hollow axis; thus showing that, while fresh matter is continually depositing to supply the growth of the bone, that which had been already deposited is removed, and that this latter process takes place in the interior of the bone at a greater rate than the other does. The activity of the two vital processes of deposition and removal, or, to speak in technical language, of secretion and absorption, is, of course, proportioned to the rapidity of growth; so that, during the early periods of life, the bones participate with the soft parts of the body in the continual change and flux that is taking place within them. When the full stature of the animal is attained, these two actions probably diminish in rapidity, but still are kept up sufficiently to preserve the life of the part. As old age approaches, the removal of the earthy ingredient of bone seems to become more difficult; its proportion, therefore, to the membranous ingredient increases, and hence the bones of old animals are harder, of greater specific gravity, and more brittle than those of younger ones.

That very remarkable natural process, namely, the annual renewal of the bony horns of the stag and other animals of the deer tribe, is, perhaps, the most striking example and illustration of the circumstances necessary to the formation of bone. These horns arise from a short process or pedestal projecting from a bone forming the upper part of the skull, and called the frontal bone. At the season of the year when the horns are about to be renewed, an increase of vital action takes place in the bone, and a faint red line, indicating the presence of blood-vessels, will be perceived in making a longitudinal section of the bottom of the horn, and the base on which it stands; the situation of this red line, indicating precisely the boundary between the dead horn and the live bone; absorption of part of the bone takes place, which loosens the adhesion of the horn to it, in consequence of which this latter falls by any accidental shock which it receives. The spongy tissue of blood-vessels, which may now be

seen covering the end of the bony base, is soon entirely covered by the growth of the external skin; and this may be considered as terminating the first part of the process. Soon afterwards a small tubercle arises from the end of the bone, and presses upwards the skin which covers it; the tubercle rapidly elongates, the skin extends with it, and in the course of a few weeks it has assumed the size and shape of the future horn; in this state it is covered by the attenuated skin, which latter has pushed out an abundant growth of short fine hairs resembling the pile of velvet. Beneath this skin is a layer of blood-vessels, the diameter of some of which is equal to that of the little finger; these rest on a thin layer of dense membrane, of the same nature as that which covers ordinary bones, and called the periosteum; and within the periosteum itself is a flexible cartilage, penetrated in all directions by ramifications from the blood-vessels already mentioned.

In this state the future horn is very tender and exquisitely sensible; it bleeds when the skin is broken, and the animal often suffers much in this part from the bites of gadflies and other insects. When the cartilage has attained its full growth, ossification begins by the deposition of phosphate of lime, and goes on till the bone or horn has acquired its complete hardness. During this process, a ring of bony beads has been forming at the base of the horn, in the intervals between which the main trunks of the blood-vessels lie; these beads enlarge by the continual addition of bony matter, and in so doing compress the adjacent sides of the blood-vessels, and thus diminish the supply of blood; at length the sides of these vessels are quite squeezed together, circulation ceases, and all the soft parts die, shrivel, and dry up, and are rubbed off by the animal against the bough of a tree, leaving the dead bone, or horn, attached by its base to the frontal bone; till, after some months, the time for shedding it again comes round, when a repetition of the processes already described takes place.

Bones, even of the same animal, vary much in structure and in hardness, and no doubt in the relative proportion of their component parts, according to the situation in which they are placed, and the use to which they are put. Thus the shafts of the long bones, being wanted chiefly for support, are more or less in the form of a hollow cylinder, and the texture of the bone itself is dense and compact. Those parts of bones that form the joints or articulating surfaces by which one is hinged on to another, require a considerable space for the joint, and for the attachment of ligaments; but as a degree of strength proportioned to its thickness is not wanted, the structure becomes cellular. A similar structure is observable in the flat bones, which consist of two thin parallel tables of dense bone, having a cellular part interposed between them. Hence, in utensils made of bone, the compact cylindrical ones are generally employed, both as being stronger and admitting of a more uniform and higher polish.

The bones of animals belonging to the same general class of nature are commonly observed to have certain points of general resemblance, by which they may be distinguished from one another, and are applied by man to various uses corresponding with such differences. Thus, the bones of fishes are softer, more flexible, and contain a much larger proportion of jelly and membrane, or, which comes to the same thing, a much smaller proportion of earthy matter, than those of the mammalia or warm-blooded quadrupeds; and the bones of these latter, comparatively dense and hard as they are, fall considerably short in density and hardness of the bones of birds, which, however, are generally too small and thin to be applied to much use in the arts.

Bone undergoes, much more slowly than the soft parts of animals do, the process of spontaneous decomposition; meaning, by this term, that disintegration of a compound which takes place either by the chemical re-action of its ingredients on one another, or by means of air, moisture, and common temperatures. The bones of a human body buried in a church-yard are, perhaps, mostly consumed in twenty or thirty years; yet under favourable circumstances they will endure for a much longer time with but little change. Thus, in the charnel-house at Morat, in Switzerland, there still remain many bones of the soldiers of Charles the Bold's army, who perished there in 1438, being 401 years ago. When Sir Christopher Wren was rebuilding St. Paul's Church, after the great fire of London, the workmen, in digging for the foundation, came to the floor of a Roman temple, dedicated to the goddess Diana, on which were the horns of stags and bones of other animals. Tombs of the ancient inhabitants of this island are occasionally opened, in which are found bones that have been deposited there during many centuries; and I have the pleasure of exhibiting to you part of a carved bone spoon (discoloured and passing to a state of decomposition, it is true,) which was found in an Etrurian tomb at Vulturnum, in Italy, possibly as ancient as the foundation of Rome. In the valley of the Lea are many peat mosses, the remains of ancient forests, now covered to the depth of several feet with alluvial silt. Many of these have of late years been dug into, on occasion of making docks and other excavations; and in or upon them have been found the osseous remains of boars, stags, and other animals, which have lain there from the time that these creatures roamed wild in the immediate neighbourhood of London. Not only the remains of individuals belonging to species now extant are still found, after being buried for centuries, but the bones of species now extinct, and many of which, judging from the habits of species nearly allied to them and now living, can scarcely exist except in warm climates, are found abundantly in the British islands, and in all parts of Europe. Remains of a large animal of the ox tribe are found in Essex. Elephants, hippopotamuses, and rhinoceroses, differing in many respects from any now known to exist, are also found in the same

county, and in other places near London. Hyænas and tigers, also, of extinct species, occur in the cavern of Kirkdale, in Yorkshire, and in other caverns in the west of England ; and in certain caverns in Germany are found the remains of two species of bear, differing, in some anatomical details, from any known living species of the same genus. There is no evidence that the human race was contemporary with these creatures ; and yet, notwithstanding the enormous length of time that must have elapsed since the deposit of the animals in the places where their bones are now found, many of them are in a state apparently of almost perfect preservation. Membrane and jelly still remain in the bones ; but the oil or fat, being uncombined with earthy matter, has disappeared.

In what I have hitherto said, I have alluded very slightly to the use of bone in the arts, which was the ostensible object of the present illustration ; for I confess that I have not unwillingly been tempted to enter into the preceding physiological and other details, in order to relieve the dryness of mere technical description. In what remains I shall treat of the practical part of my subject, beginning with an inquiry into the use of bones as articles of food.

All animals that eat flesh will likewise eat bones, provided they are of a size to be easily crushed and masticated by them ; so when a lion or tiger has taken one of the smaller antelopes, I presume he devours many of the bones along with the flesh, leaving only the spine, skull, and horns. But when he has pulled down a horse or buffalo, the case is different ; the flesh alone of the animal is sufficient for an ample repast ; the leg-bones and ribs are not to be cracked by a straightforward crush of the jaw ; and the spine, from its awkward shape, as well as by reason of the strong ligaments by which its parts are bound together, may well resist the lazy efforts of an animal already satiated with food,—not to mention that the great length of the canine teeth in the larger animals of the cat kind, as well as the small number of their grinders, render the act of gnawing both difficult and unnatural to them. The half-picked carcase, therefore, falls to the share of the wolves and hyænas. The former, after tearing off the ligaments of the joints, proceed to separate the bones from each other ; and then, by gnawing, grind off the softer parts of the spongy articulating surfaces, in which they find a wholesome food. The hyæna, with far greater strength of jaw and of teeth than any other animal of his size, goes to work bodily, especially on the ribs and other flat bones, crushing them into large, splintery fragments, and swallowing them in this state, without fear of being choked or injured by their sharp points and rough edges. These two animals, therefore, (including the dog, as a sub-species of wolf,) are eminently the bone eaters ; the membranous and gelatinous matter of the bone, being dissolved out by their gastric juice from the earthy portion, undergoes the usual process of digestion ; while the latter, apparently unaltered, passes through the intestinal canal, giving to their excrements the well-known appear-

ance of half-dried mortar, and may afterwards be applied to all or any of the purposes for which bone-earth is used.

Man, the cooking animal, extracts nutriment from bones in a different way. When very hard pressed, indeed, he can stave off famine for a while, as Captain Franklin and his party did more than once in their exploratory arctic expedition, by taking bones, which even the wolves had left, and scorching them so as in some degree to subdue their hardness; and thus render it possible to gnaw and masticate them as a succedaneum for food, or, at least, as some alleviation of the agonies of famine.

But the animal matter of bones is best extracted by hot water. Every housekeeper knows that the nutritive quality of meat soups is much increased by boiling the bones together with the meat. In this way, however, only a small proportion of the food contained in the bones is made available; for part of the gelatine is with difficulty, and the membranous part is not at all, soluble in common boiling water; much even of the fat is locked up in cells of the bone, from which it cannot escape except these cells are broken into.

The solid part of the long bones contains very little soluble matter; it would, therefore, in most cases, be a matter of economy to exclude them; the advantage to be derived from them by ordinary treatment not being equal to the value of the fuel which they would require. It is from the enlarged extremities of the long bones and their articulating surfaces that the principal supply of nutritive matter is to be derived; these parts, therefore, should be sawed off from the rest and broken into pieces. From the bones of young animals thus treated, boiling water will, in two or three hours, extract the whole, or nearly the whole of the soluble matter; but, in the bones of the older animals, the gelatine seems to be in a state of condensation approaching to that in which it exists in the skin, and therefore requires the long-continued action of boiling water for its separation. By way of experiment, I had the leg-bone of an ox sawed longitudinally, and boiled for three or four hours. At the end of this time, the whole of the fat and mucus had been extracted, with part of the jelly. On applying the finger to the cellular part of the bone when wiped dry, I found the surface to be considerably sticky, and, on examining the cells, I found many of them completely filled with a transparent substance scarcely viscid, but much resembling pieces of glue that had been put to soak in cold water; by which, as every one knows, the glue swells exceedingly by absorption of the water, without, however, becoming viscid. A second boiling for three or four hours in fresh water dissolved out a considerable proportion of the gelatine; but still the surface of the bone remained sticky, many of the cells had a glazed surface, and, even after a third repetition of the boiling, only a few even of the superficial cells were quite empty. It is evident, therefore, that we cannot avail ourselves, with any regard to economy of fuel, of

the whole of the nutritive matter contained in bones by the action of boiling water applied in the common way. But by means of a digester—that is, a boiler with a steam-tight cover and a safety-valve—we can, without hazard, raise the temperature of water from  $212^{\circ}$ , its boiling point in the open air, to  $270^{\circ}$  or  $280^{\circ}$ . At a less heat than even the former of these, not only the condensed gelatine but also the membranous part of bones is dissolved, if the bones have previously been reduced to small pieces, and the undissolved residue will be found to be a friable crumbling mass, with scarcely any remains of animal matter. It appears that bone soups are thus prepared at present at some of the hospitals and military headquarters in France, and memoirs have been published, stating the advantage of making a collection of dry bones as part of the provisions of a garrison in case of siege, being a kind of food scarcely susceptible of decomposition or of destruction by rats or mice, and which would require no other magazine than simply making them into stacks and covering them with a roof of thatch or any other material. Complaints, it is true, are made of the burnt flavour which such soups are liable to have, and perhaps it may not be very easy to regulate the temperature of the water in the digester so as to avoid the empyreumatic flavour, and at the same time completely to extract from the bones the animal matter. On this account it is that another scheme has been proposed, namely, to put the bones, after soup has been made of them by boiling in the common way, into a stone trough, and then pour on them very dilute muriatic acid. By repeating this process in the cold a sufficient number of times, the whole of the earthy matter will be dissolved out, and probably without much, if any, injury to the animal matter, which will remain in the form of a porous membrane; by repeated percolations of water the acid would be washed out; or, if a little should remain, a last sprinkling with a solution of carbonate of soda would convert the acid into common salt. The membrane being now dried in the air, will acquire a horny hardness, by which it will be rendered almost incapable of spontaneous decomposition, and would probably be found to be much more easily convertible into palatable human food by the common processes of cooking than the entire bone. The plan, to say the least of it, is plausible, provided muriatic acid may be had, as it now may be, at a very small cost.

There is, however, a whole class of animals, the bones of which, without any chemical preparation, are presented to us by nature in a state capable, with very little trouble, of being converted into nourishment. I mean the whole class of fishes. The bones of these creatures contain so little earth, that, by drying and grinding them, a powder is obtained, which, when made into cakes with meal, has proved a valuable resource to the people of Norway and Sweden in times of scarcity; and some of them, by simply browning on a



gridiron, become quite friable, and, when treated with a proper quantity of pepper and salt, form a very palatable article of food.

*Trans. Soc. Arts and Man.*

**GALVANOPLASTIK ; or the Process of Cohering Copper into Plates, or other given forms, by means of Galvanic Action on Copper Solutions.** By Dr. M. H. JACOBI, Privy Councillor to the Emperor of Russia, and Member of the Royal Academy of Sciences of St. Petersburg.\*

(Continued from page 344.)

### 5.

When we have more than one pair of plates at command, we have an opportunity of combining them in different ways. In the first place I shall remark, that by combining all the zinc plates together into one parcel, and all the copper plates into another, *the operation of them is precisely the same as if only one single pair were used, whose surfaces were equal to the sum total of all the surfaces employed.* But when the copper plate of the first pair is combined with the zinc of the second, and the copper of the second with the zinc of the third, &c., and the extreme copper plate at one end of the series, and the extreme zinc plate at the other end, are united by the conducting wire, a very distinct effect is produced, differing from that obtained by the former mode of combining the metals. Fig. 14, plate viii., shews the former mode of combining the plates so that they may operate as a single pair ; and fig. 19 shews the latter mode of combining them so as to act behind one another, or in series. I also wish to refer to Figs. 15, 16, 17, and 18, as examples of combining twelve pairs in other different manners :—

Fig. 14, the combination as 1 pair of 12 times the surface.

" 15,	"	"	2 pairs	6	"	"
" 16,	"	"	3	"	4	"
" 17,	"	"	4	"	3	"
" 18,	"	"	6	"	2	"
" 19,	"	"	12	"	1	"

The performance of these different dispositions of the plates is regulated by the following laws : *By the combination of a number of pairs, behind one another, or in series, the power of the plates themselves (1) of setting the current in action, and (2) of overcoming the resistances, will be in proportion as their number is multiplied.* We

\* Translated from the German Edition.

should, therefore, from the preceding examples of combining the plates, obtain the following rates of action :—

$$\begin{array}{rcl}
 \text{Fig. 14....} & \frac{1}{4000} & = \frac{1}{1333} \\
 & \frac{12}{2} + 1000 & \\
 \text{Fig. 15....} & \frac{2.4000}{6} & = \frac{1}{1167} \\
 & \frac{3}{3} + 1000 & \\
 \text{Fig. 16....} & \frac{3.4000}{4} & = \frac{1}{1333} \\
 & \frac{4}{3} + 1000 & \\
 \text{Fig. 17....} & \frac{4.4000}{6} & = \frac{1}{1583} \\
 & \frac{3}{2} + 1000 & \\
 \text{Fig. 18....} & \frac{6.4000}{2} & = \frac{1}{3000} \\
 & \frac{12}{12} + 1000 & \\
 \text{Fig. 19....} & \frac{12.4000}{4083} & = \frac{1}{4083}
 \end{array}$$

We also here see, that by using a conducting wire 1000 feet long, we obtain the strongest action, by combining the plates into two pairs of six each; or, which is the same thing, when we use two pairs in series, each pair of which is six square inches. For other lengths of the conducting wire, it is obvious that other combinations of the battery plates would be more suitable. If, for instance, we had a conducting wire of 12,000 feet long, we should then obtain the strongest action by the combination represented by Fig. 18, where six pairs of plates, of double the unit of surface, are combined in series. It frequently happens, however, that the combination which gives the greatest effect can only be discovered by direct experiment; by calculation, however, we find, *that the most advantageous disposition is that by which the resistance of the battery*

We have given the fractions in this page as they stand in the original; but it is obvious that some of them do not agree on both sides of the equation.—EDIT.

is equal, or nearly equal, to the resistance of the conducting wire. It follows, therefore, as a practical rule, that when we wish to have a battery for all the various purposes of experiment, we must choose one with a sufficient number of pairs, and which can easily be convertible from any required number to any other required number of pairs or factors of the whole series. If, for instance, we had seventy-two pairs, we could, by proper arrangement, convert them into twelve different kinds of battery.

Notwithstanding the galvanoplastik apparatus requires no long conducting wire, I must say, that here, as decidedly as in those cases already mentioned, the resistance of the wire is directly as its length, and inversely as its thickness. *All such wires or strips of metal exercise a similar influence on the strength of galvanic currents, when we obtain the same quotient by dividing their length by the area of the transverse section; or, which comes to the same thing, when we divide the length of the wire by its weight.* If, therefore, without augmenting the battery, we wish to obtain the same degree of action through a conducting wire of double the original length, we must employ for that purpose, a wire of four times the weight of the original one.

It has also been mentioned above, that the natural *quality* of the metals exercises an influence as decidedly as the *dimensions* of the conductor, which circumstance arises from the fact, that the various metals present a greater or a less resistance to the transmission of the electricity. The following table, the numbers of which express the quantity of resistance opposite a few of the most common metals, is founded upon the excellent observations of *Professor Lenz* :—

SILVER .....	9
COPPER .....	12
GOLD .....	15
TIN .....	36
BRASS .....	40
IRON .....	66
LEAD.....	80
PLATINUM.....	84

If, therefore, we wish to compare wires of the various metals with one another, we must multiply their lengths into the numbers expressing their resistance; and divide the product by the areas of their transverse sections. Hence, also, to use copper and iron strips of similar length and breadth, so as to produce a similar influence on the strength of the currents, the latter metal must be  $5\frac{1}{2}$  times as thick as the former. I must here remark, however, that since for our present purpose the wire is never very long, the choice of the conducting metal is not of much consequence; for if no copper be at hand, we may use brass, or lead, or tin, for the uniting conductor: iron, however, in consequence of its easily rusting, and

some other disadvantages which it presents, is not an economical metal for this purpose.

## 6.

The *galvanoplastik* art depends upon the well-known remarkable properties of elastic currents, by which almost all compound matter becomes reduced into its elementary constituents; the metals especially become separated from their solutions or combinations, and restored to their genuine metallic conditions. This property of the electric currents has been known since the beginning of the present century; but however highly important had been the scientific results emanating from that property of the currents, but little attention had been paid to the conditions by which the restored metals aggregate into masses. Frequently the metals were reduced into the shape of powders, sometimes into different sized crystals, and at others into lumps or congregated masses. The sportive freaks of chance appear to have been here the only rule; or, in other words, we were completely ignorant of the laws which govern these different formations; and even now, the impediment to a true knowledge of these laws is not yet removed; but our information is so far advanced, that we know the conditions under which the copper can be immediately liberated from its solutions, and made to assume regular coherent plates of any required form.

I will first remark, that the galvanic apparatus which I have previously described, consisting of zinc, copper, and an acid, is not applicable to technical, or to several scientific purposes. They are very changeable in their efficiency, and their powers, after a short time, became much enfeebled. However, these impediments are met by batteries of more modern date, which have come into general use. The dissimilar metals, of these latter apparatus, are not immersed in one and the same liquid, but in two different liquids, which are separated from each other by a porous wall or diaphragm, which the electric current percolates. The order of arrangement of such an apparatus is represented in fig. 20, where *a, b, c, d*, is a reservoir which, by a porous diaphragm, is separated into two cells. One of these cells is filled with a diluted acid, or saline solution, in which the zinc plate *Z* is immersed; in the other cell the copper plate *K* is placed with a solution of sulphate of copper, or some other cupreous salt. Both plates are joined by a conductor, and form a galvanic circuit.

It will be remembered from what has already been said, that with the common apparatus hydrogen is developed on the surface of the copper plate, and the zinc dissolves in the acid solution, so long as the circuit is closed. The dissolution of the zinc also takes place in the new apparatus; but the hydrogen does not appear on the copper plate, for it returns back and liberates the copper, in a metallic state, from the solution, and reduces it on the surface of the copper plate of the apparatus. Notwithstanding the difference in the con-

stancy of action of this form of battery and the common one, however, which, by the former may be continued in uninterrupted activity for days or weeks together, the same laws of action, already discussed and explained, apply to both kinds of apparatus. In the year 1837, whilst in Dorpart, I had a series of experiments to carry on, upon the strength and duration of galvanic currents produced by an apparatus constructed upon similar principles; but instead of copper plates I furnished myself with copper cylinders, surrounded by animal bladders, for the purpose of keeping the liquids separate. As these bladders became damaged by use, they were taken out, and gave occasion to inquire into the form in which the copper was reduced. It was found on the surface of the copper cylinders, and in the inner folds of the bladders, partly in thin bars and partly in large and small corns of a crystalline texture, which to those beneath shewed not the least attachment. Afterwards, however, whilst continuing to remove these corns, &c., it was found that they adhered more closely together, and required some force to separate them: it was also found that the copper cylinder itself was completely covered with a layer of reduced copper, which, to my astonishment, was removed in large well-connected plates. As no mention had hitherto been made of such regular formations of reduced copper, these corns, &c., were held of a high interest. I must confess, indeed, that I was myself surprised, as I remarked at the time, that some fine file marks and indentations from hammer-blows, which were conspicuous on the surface of the copper cylinders, had, with the greatest degree of accuracy, given corresponding forms to the plates of reduced copper. This remarkable phenomenon was a strong proof of the conformation to the law by which copper is capable of being reduced, and which could not have been expected from the ununiform productions which had previously been obtained. As the reduced plates acquire a certain degree of connection and firmness, there were hopes that, by a discernment in their management, these properties might be obtained in a still higher degree: and, finally, this humid method of forming copper plates on the surface of the cylinders, by galvanic action, immediately held out a practical result worthy of pursuing. How and by what manifold experiments these expectations shall be conformed with, and what will be acquired, in the course of performance, beyond that already done, cannot, in this place, be mentioned, where merely the description of the proceeding and the methods hitherto given are intended.

It appears, therefore, that firm coherent copper may be reduced from its solution by the galvanic current. For this purpose we employ an apparatus similar to that formerly described. It is represented by fig. 21, where *a*, *b*, *c*, *d*, is a wooden trough or reservoir, made completely water-tight by a lining of asphaltum or of pitch, which serves also to guard the wood from the attacks of the acid. In the sides of this trough, and directly opposite each other,

are two grooves, for the reception of a plate (*ef*), of slightly burnt clay or unglazed porcelain, which is firmly cemented in its place, so that the two divisions of the trough, which it forms, may have no communication with each other. This partition must be of such a nature that it will permit a free passage for the electricity, although it must allow but a slight connection between the two liquids; and care must be taken that it does not become loosened or destroyed by the action of those liquids. I had, in the first instance, much trouble in procuring the necessary porous plates for the diaphragm; but at present I can get them of superior goodness at the stone-ware manufactory (*Guntherschen Steingutfabrik*) at this place. I also considered attentively on the possibility of employing lime-tree, or some other porous wood, instead of clay, for the diaphragm plates, and proposed to myself some experiments for my instruction; but as these experiments were not continuously carried on, I am not prepared to shew whether or no any disadvantage would attend the employment of wooden diaphragms. The wood was previously boiled, for an hour at least, in water containing a little sulphuric acid. The liquid which I used in the copper cell, was a solution of sulphate of copper, well saturated by dissolving the salt in hot water. Since, however, in time, the solution must become feeblery by the reduction of the copper, it is necessary to be prepared with a suitable stock of bruised sulphate of copper, to add to the liquid and keep up the point of saturation, and thus replace the copper which has previously left the solution. For this purpose we may employ a square box, *k*, fig. 21, made of thin boards, and pierced with several holes in the bottom and sides. In this box the bruised salt is to be placed, and hung in the cell of the reservoir, so that the liquid may wash the upper strata of the sulphate, where it is always less saturated than below.

For the liquid in the zinc cell we use diluted sulphuric acid, or a solution of sal-ammoniac, common salt, glauber salts, &c. In all cases it is well to amalgamate the zinc plate; however, this is less necessary when solutions of any of the neutral salts are employed. This liquid must, now and then, be renewed, or at least be diluted with water; for, after a short time, it becomes pretty well charged with dissolved zinc, and finally crystallizes. For the purpose of discharging the liquids, such cell or compartment is furnished with a bent tube, as represented in the figure. These tubes are furnished with pliable caoutchouc joints, which are found very convenient.

The size of the trough or reservoir necessarily depends, chiefly, on the size of plates; however, it is well not to make it too small, because when we have a good quantity of liquid to operate on, the action is more equable and constant than it otherwise would be. Moreover, there is an advantage in having the reservoir of some considerable length, in consequence of having an opportunity of moving the plates to a greater or less distance from one another, so as to be able to regulate the action. We have seen, in a former

part of this work, that the strength of the galvanic current is measured by the quantity of hydrogen gas developed on the surface of the copper plate ; but, by the present arrangement, the strength of the current is shewn by the quantity of copper which it reduces on the surface of the copper plate, and, consequently, takes away from the liquid. In order, however, to be certain of success, the liquid must always be kept completely saturated ; to attain which, we must observe, that no more copper is reduced than is restored again by the dissolution of the bruised crystals in the perforated box ; or, in other words, that the strength of the current with respect to the surface of the plates, should observe a fixed ratio to the dissolution of the sulphate of copper. A very exact indication of the process of reduction with respect to the recognised regularity with which it ought to proceed, is the light-red flesh colour of the reduced copper itself, which cannot easily be mistaken by any one who has once seen it. As soon as this colour gets darker, and begins to have a sooty brown colour, it is a sign that the solution is not retained at the required degree of saturation. In this case it is best to enfeeble the current, which, as we have seen above, can be accomplished by lengthening the conducting wire, or with the same length, but thinner ; by removing the plates to a greater distance from one another, or, finally, by diluting the battery liquid with more water. Whichever of these means we avail ourselves of, the result on the current will be the same. In reference to the duration of action, it is advantageous to have a large quantity of the fluid, and to keep the plates at a considerable distance from one another. If the current be weakened by diluting the liquid in which the zinc is placed, it will be convenient first to diminish the solution of zinc previously formed. Now, although the precipitated copper is firmer and tougher, by a feeble current, or by slow reduction, there is a certain limit at which no reduction takes place ; and, on the other hand, there is also a practical view to be regarded, which makes it desirable to hasten the process as much as possible. Much may be accomplished on these points, by varying the dimensions of the perforated reservoir *k*, and the application of heat. It hardly need be mentioned, that instead of a wood box, one of glass or porcelain would be preferable ; and it is not impossible that metal might be generally employed.

Instead of the before-mentioned reservoir, with a fixed partition, we may employ an earthenware vessel, which may be either round or flat-sided, as circumstances require. With a rectangular one, we may have three cells, and either one zinc plate and two copper plates, or one of copper and two of zinc. Fig. 22 shews this arrangement, where *a, b, c, d*, is the reservoir, and *e, f, g, h*, the porous earthen or clay diaphragm or vessel, and *Z, K*, are the zinc and copper plates. I will observe, that the manufacture of rectangular porous vessels requires some trouble ; but I was enabled to get them made, after some tedious labour which was bestowed upon them. They were burnt in a muffle.

Instead of placing the plates vertically, we may place them in a horizontal position, as shown by fig. 23, where *a, b, c, d*, is the reservoir; *e, f, g, h*, the porous floor or diaphragm; and *K, Z*, the copper and zinc plates respectively. We may reverse the order of the plates, and have the copper above and the zinc below the diaphragm; but this latter arrangement is not recommended, because it is subject to many practical disadvantages and inconveniences.

Instead of the separation of the two liquids by potters' clay or wood, we may, with more or less advantage, employ various other substances, such as stout bladder, English brown paper, leather, parchment, thick and close woven cloth, &c.

It is of the highest importance that we, in every stage of the process, are enabled to ascertain the precise degree of action in the apparatus. For this purpose, I usually employ a galvanometer, and ascertain the degree of action, and consequently the rate at which the copper reduces, by the deflection of the needle. This instrument may be constructed in a very simple manner, as shewn in fig. 24, where *a, b*, is a round board of hard and dry wood, furnished with three screws for adjusting it to the horizontal position. In the centre of the board is fixed a fine steel point *C*, on which is freely suspended a magnetic needle. Under the middle of the board is fixed a copper wire of about  $1\frac{1}{2}$  or 2 lines in diameter, the ends of which are furnished with two mercury cups, *d* and *e*: on the upper surface of the board is a graduated circle. The wire must pass directly under the steel point which supports the needle, and must be placed lengthwise in the magnetic meridian, when the needle stands at zero. If we connect the excited plates of the battery with the wire in the cups *d, e*, the needle immediately becomes deflected, and after several oscillations, will settle on some of the divisions on the scale. The diminution of this deflection shows a corresponding diminution of power in the battery. In this case we must restore the saturation of the cupreous solution, either by renewing it altogether, or by means of the magazine of crystals of sulphate of copper; or sometimes an accidental circumstance takes place which renews the action. Frequently the diminution of power is occasioned by the diaphragm being stopped with impurities, &c., in which case it must be removed, and completely cleansed from the saline matter which lodges in its pores. When the dimensions of the apparatus are considerable, we may convince ourselves of its activity by the spark which is produced on opening the circuit, or by its power of rendering a thin platinum wire red hot. However, these indications are always more uncertain than those exhibited by the galvanometer needle.

(To be continued.)



*Report on a Memoir of M. MALLET, Professor of Chemistry at St. Quentin, on a Process for the Purification of Gas used for Lighting.\** Commissioners: M. M. PELOUZE, BOUSSINGAULT. DUMAS, reporter.

The Academy has charged M. M. Pelouze, Boussingault, and myself, to render an account of a memoir of M. Mallet, relative to the refinement of the gas used for lighting, produced from oil: we have just accomplished this duty.

It is well known to every one what inconveniences occur from the presence of pyrites by which oil is always accompanied, caused in the fabrication of gas. The sulphur of this pyrite produces sulphuretted hydrogen gas, which, combined with ammoniacal gas, renders the gas very deleterious, a circumstance easily proved wherever the gas is observed. When the sulpho-hydrate of ammonia arrives with this gas at the lamp where combustion is effected, we know that the sulphur ought to be converted into sulphurous gas, whence result new and serious inconveniences. In fact, the sulphurous gas exercises on stuffs a grievous and vexatious action, destructive of certain colours, and it produces on the lungs an irritation which at length becomes serious in its consequences.

Up to the present period we have been content to absorb the sulphuretted hydrogen by means of a milk of lime. This process is expensive for the producers, for the product which results from it is of no value. It follows very naturally that the manufacturers, who are desirous of proceeding with economy, too rarely renew their milk of lime, and suffer to pass thereby a portion of the sulphuretted hydrogen without absorbing it. In the arts, all researches having for their object the means of salubrity or health, ought to be based upon a fundamental principle, without which there is no guarantee for an extended and durable application. It is necessary that the process of purification become a source of revenue. If it is beneficial in that point of view, policy will not be long in taking advantage of it; if it is attended with loss, its action will not always be very efficacious, and the inspection of it ought never to slumber.

M. Mallet employs a process which produces a benefit, and it is in that point that it is good, and that its merit has been signalized.

The author himself being convinced, on one hand, that the burnt gas contains sulpho-hydrate and cyan-hydrate of ammonia, and on the other hand, that when we purify it by lime, this latter does not fix the acids, leaving the ammonia free, he has it in his power more conveniently to effect a system of purification.

In fact, that which renders so difficult the separation of the last traces of sulphuretted hydrogen by lime, is precisely owing to the existence of ammonia in the gas in great excess, in which they are combined.

\* Comptes Rendu, Aug. 16th, 1841.

The author has also overturned the system of purifying. He commences by seizing the ammonia by means of salt of iron, or of manganese; and the portion of sulphuretted hydrogen which escapes at this first condensation, is afterwards very easily retained by a milk of lime.

The sal-ammoniac formed in the first washings, being collected, constitutes a real benefit for the manufacturer. The expenditure in lime is, besides, very much reduced, the major part of the sulphur being again recovered in the state of sulphate of iron, or of manganese.

Your reporter has seen the process, which is in full action at St. Quentin.

The gas of oil, which is thus fabricated, becomes for lamps entirely divested of sulphuretted hydrogen or ammonia. It only retains the odour of volatile oils, which the distillation developes.

These facts, which have been well proved, of which we have just been speaking, and the well discussed principle on which the process of M. Mallet is founded, have decided the commission to propose to you to send his memoir back again to the Commissioner Montyon for the Insalubrious Arts.

The conclusions of this report were adopted.

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*An Account of a Voltaic Battery.* By Mr. JAMES YOUNG, late  
Chemical Assistant in the Andersonian University.

It is now about six years since this battery was made known through the medium of the *Philosophical Magazine*; but being about to give a general history of every kind of voltaic apparatus that we can get information of, we give Mr. Young's battery a place at this time.

The sheet copper and sheet zinc to be used in this battery are first cut into long ribbons, of the breadth which it is intended to give the plates. Suppose the ribbons two inches broad; both the copper and zinc ribbons are then divided into lengths of five inches, and a portion cut out as in fig. 1. The slip is thus divided into two squares of two inches each, which are connected at A, and a piece is left projecting at B. The zinc and copper sheets are cut up exactly in the same way. Fig. 1 therefore represents either a single zinc or a single copper plate. The plate is then bent at A, and presents the appearance represented in fig. 2. In fig. 3, we have two plates, one of copper C, and the other of zinc Z, which are exactly alike in construction, but are placed differently, as shewn in the figure. Thin projecting parts B B are soldered together, and this is the only metallic communication between them which is allowed to exist. Fig. 3, therefore, is only one copper and one zinc plate, or it is one pair of plates. Each pair is made up in the same

way. In arranging a number of pairs to form a battery, they are interlaced so that a copper square comes in between each couple of zinc squares, and a zinc square between each couple of copper squares. It is easy to see how this arrangement can be made, when the plates are in the hand, though it is difficult to describe it. At the positive end of the battery there is a single copper plate, which is soldered at the top to the last double copper plate, as seen in fig. 4; which figure represents three pairs properly arranged, and also the manner in which they should be fitted up and kept steadily apart in a wooden frame. This frame consists of two cross-bars, *e e*, *e' e'*, in front, and the same behind, dovetailed into solid ends. The channels in the cross-bars, for the reception of the edges of the plates, are formed by placing the four cross-bars together, and sawing a little way into one side of them all, every eighth of an inch or so in their length, so as to form a set of parallel grooves. We have by means of this frame a much greater security that no metallic contact will occur between contiguous plates, than when they are separated by wedges of cork, as in Dr. Hare's construction, which may slip out.

The frame and plates are introduced into a trough, which may be of wood or stoneware, containing the exciting liquor. Dr. Hare's revolving arrangement of the two connected troughs may be adopted for this battery, although we have been led to give a preference in practice to a single trough to contain the frame. To the solid ends of the frame are attached two cords, which are fixed to two pulleys, on which they are wound up, on turning a winch, as represented in fig. 5, by which means the frame and battery can be raised out of the fluid. If the axis (a stout wire) on which these pulleys are fixed can be moved a little backwards and forwards on its bearings, it is easy, by means of a little projecting peg at *P*, which fits into a hole in the side of the pulley, to fix and support the frame in a position above the trough, and out of the exciting fluid, when that is desirable. But the form of the trough to contain the frame and plates may be varied according to the object in view, or the purposes to which the battery is to be applied.

In comparing a battery of the form described above, either with Dr. Hare's or any of the other forms in use, it is to be remembered that the plates or elements of the battery are all of double the size they appear to be, or that in this construction you have half the number of pairs, but each of double the dimensions of a pair in any of the old batteries having the same appearance.

A small battery of this construction, containing twelve pairs, of two inches breadth, of plates the size which we have taken above as an example, may be contained in a trough eight inches in length, and will evolve, when its terminal wires are soldered to a Faraday's volta-electrometer, six or seven cubic inches of the mixed gases in three or four minutes, with a charge of half an ounce of sulphuric acid and half an ounce of nitric acid, in twenty-four

ounces of water, (all by fluid measure,) and is therefore amply sufficient to demonstrate the decomposition of water on a considerable scale.

It is proper to use the thickest sheet zinc which can be had, in the construction of the plates, although the thinnest sheet copper will suffice, from its being so well supported. When the zinc plates are worn out, the cross bars may easily be pulled out of the solid ends, and the elements of the battery separated. New zinc plates being soldered to the old coppers, the whole may again be quickly rearranged in the old frame.

*Glasgow, Jan. 4, 1837.*

## ELEMENTARY LECTURES ON ELECTRICITY, &c.

### LECTURE XIII.

(Continued from page 420.)

previous experiment; but you may possibly have perceived a difference in the character of the report in this case, and in those cases when no paper was present; it is, in fact, usually a much duller sound than is produced by a discharge through several plies of paper, than that which occurs when the circuit is all metallic. Now let us examine the paper, and we shall find that every slip is perforated, and although the perforations are exceedingly small, you will be enabled to see through them very distinctly. And if you examine the pieces of paper separately, you will probably find some of them perforated in two or more places.

Now, in this case we have an effect produced similar to that produced by pushing a pin, or rather a very fine needle, through the paper, but by the latter mode of perforation a force must be used; that is, the force of the hand, which everybody would acknowledge to be a mechanical force, or perhaps they would rather contrive to say that the *effect* produced was mechanical. We will conclude then that the perforation of the paper in this case is also mechanical; and the force itself electrical.

We may now try the electric force of the jar upon a couple or more of cards; and at the same time let us measure the extent of the charge by Henly's electrometer: by which means we shall be enabled to form some idea of the relative effects produced by employing charges of different degrees of power.

We will first charge the jar till the index of the electrometer stands at forty-five degrees, and we will discharge the jar on to three cards formed into a pile. The noise made by this discharge is similar to that when no paper is in the way: the fact is, the fluid has jumped from the lower half of the discharging rod to the tin foil, without passing through the cards, so of course they are not per-

forated. Now this circumstance may have arisen from either one or the other of two causes. The electric power may have been too feeble, or the cards too narrow. Indeed there is another circumstance which may have interposed. It is quite possible that the upper card may be a little damp, and thus conduct the fluid to the edge of the pile, where it would find but little resistance by the thin intervening stratum of air between the edge of the upper card and the metallic foil.

Let us try a similar force on one card only. You see that it is perforated, and the hole is much larger than any of those made in the pile of paper. Now, from the result of this experiment, there is a strong probability that the charge measured by forty-five degrees was not strong enough to perforate the three cards, so that we will try with a higher charge. We will charge the jar to  $80^{\circ}$ , and fire again on the pile of three cards. Now, if you were looking at the cards, you might see the fluid dart over the edge of the pile, so that none of them have suffered perforation. Now the probability is, that the upper card is damp. Let it be dried at the fire, and replace it on the pile. Now fire again from an  $80^{\circ}$ 's charge: the cards are all perforated.

By making an extensive series of experiments of this kind, on cards, paper, &c., we find many curious effects produced. In some cases the fluid passes through some of the plies and not through the rest. In several instances the cards are curiously torn, and in all cases a peculiar odour, resembling that from phosphorus, is left on every piece of card or paper operated on.

We may now try the electro-mechanical powers on some other kinds of matter. Let us take a piece of very stiff pipe-clay, roll it into the shape of a cylinder, and introduce two wires to its axis, one at each end, and push them into the clay till they are within about one-eighth of an inch of meeting each other in the centre of the cylinder. Now it is very obvious, that if the clay between the inner ends of the wires does not offer too great a resistance, the discharge from the jar will pass through the cylinder. We will charge the jar to  $80^{\circ}$ , and now, by discharging it by the route I have pointed out, you find that the pipe-clay is no longer of a cylindrical form, but has bulged out at the centre round the opening between the wires. This effect is occasioned partly by the expansion of a small quantity of air in that part of the clay, partly by the direct expansion of the electric fluid, and possibly by the conversion of a small portion of the moisture into steam.

There is a very interesting electro-mechanical experiment to be made by an instrument called the electric mortar: it is the invention of Father Baccaria. This pretty little apparatus is an ivory model of a military mortar, such as is employed for throwing bomb-shells. The ball is also of ivory, and reaches only a little way down the piece. The bottom of the bore of the mortar is conical, with the base towards the muzzle. Through opposite sides of the mortar,

near the breach, are passed two small brass wires, which almost, but not quite, meet in the bottom of the bore. One of these wires is connected, by means of a brass chain, with the outside of the jar, and the other with one branch of the discharging-rod. Now, all being ready, that is, the ball in the mortar, and the jar charged to  $80^{\circ}$ , or upwards,—fire. You see the flash of electric light, and hear the report as usual, but the ball has not left the mortar. This is often the case under these circumstances; but if we vary the experiment, by merely moistening the bottom of the mortar's bore with a drop of water, between the inner points of the wires, we shall succeed in propelling the ball even to a considerable distance—twenty or more yards.

Now I think it is pretty obvious, in this case, that a portion of the moisture was converted into steam, whose expansive power had much to do in projecting the ball. It is obvious, also, that since a moderate sized jar, such as we have hitherto used, will project a ball to so great a distance, a larger jar, or, still better, several jars properly arranged, in a manner which I shall explain in the next lecture, when discharged in a similar manner through a small portion of water, would be the means of suddenly generating a sufficiency of steam to project a large ball to a great distance indeed.

On the same principle, we very frequently make an experiment with a model of a house, or sometimes with a model of a tall obelisk. The obelisk is made of several pieces of dry wood, piled on one another, tapering from nearly the bottom to the top; and each piece is furnished with a vertical wire, in such a manner that when the obelisk is complete the series of small wires make a complete conductor from top to bottom of the model. Now when the lower end of this conductor is connected with the outside of the jar, and the discharging rod forms the communication with the inside and upper part of the conductor, the electric fluid runs down the wires without disturbing the model: and, indeed, this series of wires would conduct a very heavy discharge, even from an extensively charged surface of glass.

Now the model is so made that the security of the greater part of it depends on the position of one small piece near the base; which, if disturbed or thrown out of its place, the whole of the obelisk above it would fall down. This key-stone is a thin square piece, which fits into its hole, whether its wire be in the alinement of conductors or at right angles to it, and whether the wire side be turned outwards or inwards. Now, when the discharge is intended to knock down the obelisk, this important piece is placed in the square hole with its wire inwards, and at right angles to the other parts of the conductor; consequently there are two breaches in the circuit, one on each side of that wire, and those also behind the piece. If, now, a discharge be sent through this circuit, two explosions occur behind this key-stone; and if the discharge be sufficiently powerful, the piece is blown out of its place, and the whole fabric tumbles

down. This effect is much more certain when a little moisture is placed on the inner surface of the piece intended to be blown out, or on the wood behind it. When the apparatus is the model of a house, or other building, a similar contrivance is formed in it for the purpose of showing the possibility of such buildings being destroyed by discharges of lightning. It is the invention of Dr. Franklin.

Another exemplification of the electro-mechanical effects produced by the discharge of a jar, is that of breaking lump sugar : if, for instance, we send the discharge through a piece of sugar, it will be broken into a great number of small fragments, and scattered in all directions ; and when the experiment is made in the dark, you will observe that every fragment has become illuminated with something like a feeble phosphorescent light ; and, as in other cases of electric discharges, the peculiar odour is developed on every fragment of the sugar.

By discharging the jar through a thin piece of resin, or hard sealing wax, similar mechanical effects to those on the sugar are produced, but the phosphorescent light does not appear. We can break common window glass very easily by similar electro-mechanical powers. To prove this fact, let us place a narrow strip of gold leaf between two slips of glass, and having placed something heavy on the upper piece when laid on the table, we will connect one end of the gold leaf to the outside of the jar, and by means of the discharging rod, transmit the discharge to the other end : the glass is broken to pieces, and the gold leaf has disappeared, being consumed or converted into an oxyde. Some of the gold, however, is absolutely forced into the glass in a pure metallic form. By a similar process, on a large scale, we are enabled to produce beautiful permanent specimens of the electro-mechanical powers.

Another method of breaking the glass, is by placing two thin wires between two pieces of glass, in such a manner that they point towards each other, in the middle of the glass, without being in contact. When the discharge is sent through these wires, an explosion takes place at the opening, and if the glass be hard pressed down, by a weight or otherwise, it is generally broken into small fragments : even the weight itself is agitated by this process.

This last effect has given rise to the idea of forming an experiment, the effects of which should be imitative of those of an earthquake. A few small light models of houses are placed on a flat slab of ivory, which covers the two wires through which the discharge is transmitted, and the explosion between the inner points of those wires shakes the ivory ; and its little town, when properly built, is overthrown by the shock. When this experiment is made on a large scale, it has certainly a very imposing effect.

If we insert two pointed wires in a thin piece of deal wood, with a small space between them, a discharge from a large jar will splinter

the wood ; but much precaution is required to succeed with this experiment.

In all the electro-mechanical operations, the effects are enhanced by confining the action to within narrow limits, or by concentrating the electric fluid in that part of the circuit where those effects are intended to be produced ; and I know of no instance of electric action which better illustrates this position than that produced by the experiment I am now about to offer to your notice.

In a small glass phial, containing sweet oil, I place a finely-pointed brass wire, which passes through the axis of the cork. The upper end of the wire is formed into a ring (a ball would answer quite as well), and the lower end, just above the point, is bent at a right angle, so that the point can be brought into contact with the inner surface of the phial, below the surface of the oil. I now hold the phial in my hand, and place the thumb against that part of the glass nearest to the point of the wire. With this position of the wire and thumb, I take a few sparks from the prime conductor to the ring of the wire, and these sparks absolutely pass through the glass, and produce a pungent pricking sensation on the thumb. By turning the wire, so that the point may touch another part of the glass, another perforation is immediately produced ; and they may be made sufficiently large to permit the oil to pass through them. Now, the oil being a non-conductor, and the wire reduced to a point, the sparks become much condensed at that part of the circuit, and produce a mechanical effect on the glass, which cannot be accomplished when the point is surrounded with air, which gives way by the slightest explosion, and allows of an attenuation of the electric force.

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## PRIZE VOLUMES OF THE ANNALS OF ELECTRICITY, &c.

*Subjects for Prize Volumes before June, 1842.*

1st. For a description, with figures, of an electro-magnetic engine that will drag a ton weight on a railroad, at the rate of ten miles an hour. The engine may be either stationary or locomotive.

2nd. For a voltaic battery that will decompose acidulated water at a greater rate, and with less cost, than by any hitherto made public. The time of action not to be less than four hours.

3rd. For any kind of magnet that will support 100lbs. at two inches distance from its poles.

4th. For the best experimental investigations in any branch of electricity or magnetism.

Candidates for *prize volumes* are requested to send in their papers on or before the 15th May, 1842.



The prize volumes are bound in maroon-coloured Turkey morocco leather, highly ornamented with gold. On the front side of the cover is an emblem of the object of the work, consisting of the following group:—The centre is occupied by the exterior part of Mr. Weekes's atmospheric electrical apparatus, consisting of two church spires, from which hangs the exploring wire over a part of the town of Sandwich. This formidable apparatus is flanked, on one hand, by a plate electrical machine, and, on the other, by a voltaic battery and a magnet rotating on its axis. The lower part of the group consists of the three singular electro-magnets of Messrs. Joule, Radford, and Roberts. In various parts of the emblem are seen a Leyden jar, discharging rod, crucible, and retort. The whole is surmounted by a thunder-cloud, in which is seen the Temple of Science; and, close below, floats an electric kite, the strings of which descends to the jar. The whole group rests on the coneave of a crescental band, carrying the motto, "*Naturæ interpret vis electrica*," and, being enriched with gold, has a very good effect. On the back part of the cover, in good letters, is the name of the individual to whom the volume is awarded, and also the subject of the prize.

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*Application of Gilding in the Moist Way to the Art of Engraving.*

(Extract of a letter from M. De la Rive to M. Dumas.)

Mr. Hamman, an engraver in our city (Geneva), has just made an interesting application of my process of gilding, to the art of engraving, by aquafortis. He gilds the plate of copper designed to receive the engraving, instead of covering it with wax, and then traces upon its surface the lines of his design, removing the gold wherever the point passes. He then spreads over it the aquafortis, which attacks and corrodes the copper wherever it is exposed. This process seems to have many advantages over that in which wax is employed. In the first place, the coating of gold being permanent, the plate may be corrected if the proof show faults: in the other process, when the wax is removed, it is very difficult to make any corrections. Moreover, the lines which are traced upon the coating of gold may be made much more fine and delicate than those which we find upon wax. It appears that gilding with mercury, besides being much more expensive, does not present the same advantages and facilities in application.

*Annales de Chimie et Physique.*

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*Hourly Meteorological Observations in India.*

Mr. CALDECOT gave an account of the observatory erected at Trevandrum, by the Rajah of Trevancore, one of the native Indian

Princes, and of the instruments already placed, and of those which he had been already authorised to place there. The hourly meteorological observations have been made since June, 1837.

The registers of the barometer give the times of maxima of daily pressure between the hours of 9 and 10, A. M. and P. M., and of minima between 3 and 4, P. M. and A. M.

Fall of the barometer between 10 A. M. and 4 P. M. 0.109 inch.

Rise       "               "               4 P. M. and 10 A. M. 0.108 "

Fall       "               "               10 A. M. and 4 A. M. 0.071 "

Rise       "               "               4 A. M. and 10 A. M. 0.073 "

### EDITORIAL NOTICES.

The eighth annual report of the Royal Cornwall Polytechnic Society, and the first part of the Memoirs of the American Academy, have come to hand, and will be more particularly noticed in the next number. The former work evinces a growing importance of the Royal Cornwall Polytechnic Society, and the latter contains as interesting a series of magnetic observations, by Professors Lovering and Bond, as any hitherto submitted to the public. We hope to be enabled to show specimens of the interesting character of both works in our next number.

We have also received the first seven numbers of the Italian journal, "*Annali di Fisica, Chimica e Matematiche*," by Professor G. A. Majocchi, a work which promises fair to be well calculated for the dissemination of science.

Having had several applications to reprint, in the *Annals*, certain papers of ours which appeared in the *Philosophical Magazine*, prior to the commencement of this journal, we now promise to transplant the whole of them, with corrections, in the order in which they originally appeared. The first will appear in the January number.

We have also been requested, by various parties, to publish such a general outline of experimental Electricity, Galvanism, and Electro-magnetism, as might be suitable for beginners. We have long ago promised to do something of the kind, and have had much of the materials collected and arranged. We consider, however, that a mere *outline* is not that which is wanted. We, therefore, now propose to publish a "*CABINET CYCLOPEDIA OF EXPERIMENTAL ELECTRICITY AND MAGNETISM*," in all their departments; which will comprehend mechanical-galvanic-voltaic-magnetic and thermo-electricity; magnetism, electro-magnetism, and electro-chemistry. The work will occupy about six small volumes, the first of which will appear in February, 1842.

Fig 1.

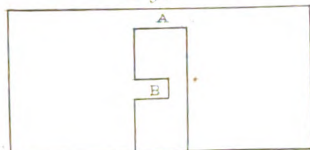


Fig 2.



Fig 3.

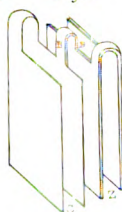


Fig 4.

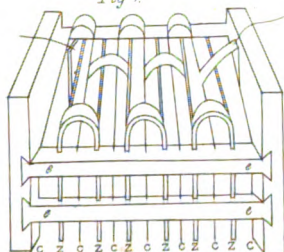


Fig 5.

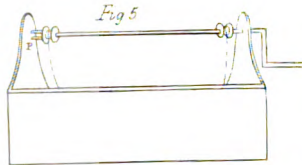
*M<sup>r</sup> Youngs Voltaic Battery*

Fig 6.

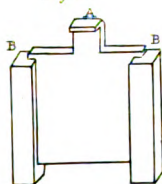


Fig 7.

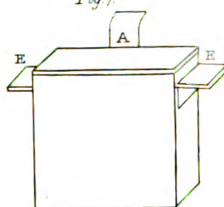


Fig 8.

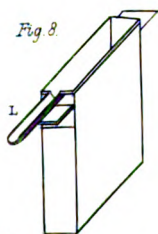


Fig 9.

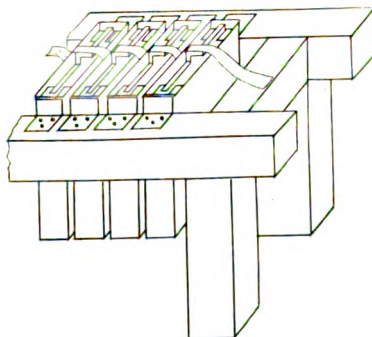


Fig 10.

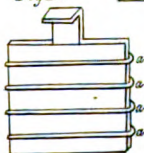


Fig 11.

*M<sup>r</sup> De la Rue's Voltaic Battery*







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